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THE PSYCHOLOGICAL REVIEW

DUALISM AND GESTALT PSYCHOLOGY

BY GRACE A. DE LAGUNA

Bryn Mawr College

E. Meyerson remarks in 'Identité et Réalité' that a scientific theory is never abandoned because of the weaknesses it develops in the course of time. These are patched up so far as possible, and ignored when they can no longer be repaired. But since some sort of theoretical structure is necessary in order that the work of science may be carried on, the old theory, however inadequate, is retained until a new theory is forthcoming with which to replace it. The *Gestalt* school would offer us such a new theory.

The difficulties and inconsistencies inherent in the theoretical tradition of American psychology have not been altogether concealed from the eyes of either American critics or American experimentalists themselves. That the atomistic doctrine of elements, inherited from the school of Locke and Hume, is wholly incompetent to explain even simple perceptual experience has been admitted freely for many years. No one seriously supposed that sensations had any ontological status —they were mere 'artifacts,' 'products of abstraction,' 'heuristic devices,' etc. And as to association, was it not borne in upon those of us who were students some twenty-odd years ago that it was only 'meanings' that were associated and not the sensory elements themselves? 'Meaning' indeed has been the sovereign remedy for all theoretic ills, prescribed freely in all emergencies. How it worked its cures for the

maladies of the moribund classic theory, no one, I think, has ever been quite clear. One simply took it on faith like any nostrum. Meanwhile the whole classic theory has been attacked from time to time by critics on the outside, and many mortal blows have been struck, but the corpse has still survived. As Köhler remarks ('Gestalt Psychology,' p. 373), "the well-established and socially acknowledged concepts of experimental psychology belong almost completely to the theoretical world of Hume." Just so. Research must continue to go on, and it must perforce be guided by theoretical concepts, and so it has continued to use those sanctified by long tradition. What makes the attack of Köhler and his associates so notable is the fact that it is backed both by rich experimental achievements and by a procedure based on new theoretical presuppositions. Most of the criticisms of theory have indeed been made before; many of the fundamental principles of which the positive doctrine makes use have been formulated by others; a good many of the experimental results have been previously reached. But the combination is new and original. No one can read of the investigations of this school and follow its discussions without feeling that here is a breath of new life.

In his recent semi-popular 'Gestalt Psychology,' written especially for the American public, Köhler has set forth his doctrines in systematic opposition to what he regards as dominant American theory. The American psychologist presented in these pages appears, it must be confessed, as a sort of scientific Babbitt. The picture sketched perhaps resembles its original about as closely as Sinclair's Babbitt resembles the typical American business man. Nevertheless, if the features depicted compose to a total caricature, they are severally faithful likenesses of well-known traits of present-day psychology. The present writer, for one, has no wish to defend the doctrines that Köhler has attacked. On various points of the indictment they may well be defensible, and they will no doubt find able champions. What inspires the present discussion is interest in the positive doctrines of *Gestalt* theory, especially as they are presented in this work. If more of

questioning than of acceptance appears in what follows, that is because it seems worth while to discuss only what one finds questionable. It is in fact just because the writer finds so much to sympathize and agree with in Köhler's views that it becomes urgent to formulate difficulties and disagreements. Moreover, there are certain theoretical possibilities which Köhler seems to have overlooked, and which to the present writer loom very large.

1. Köhler condemns equally both introspectionism and behaviorism. Behaviorism is justified in his view in so far as it attacks the method of introspection, as actually practiced, and the results it has attained. The method is sterile, and the pursuit of it has led psychologists into blind alleys of research. But the real difficulty, Köhler urges, is not at all what the behaviorist has taken it to be; namely, the inherent futility of attempting to observe 'direct' experience as such; but rather the whole set of theoretical presuppositions as to what constitutes direct experience. It is these presuppositions that are at fault, and precisely because they permit the pyschologist who accepts them to find only discrete elements when he uses introspection. These same preconceptions are, as a matter of fact, shared by the behaviorist himself, for he accepts the same type of 'machine theory' of the nervous system as does the introspectionist. What really leads the behaviorist thus mistakenly to condemn all observation and description of direct experience is his narrow and one-sided epistemological 'purism.' He utterly fails to appreciate the fact that all science, physics as well as psychology, must necessarily rest on direct experience, and this not only our own but others' as well. Hence he inconsistently rejects in psychology what he perforce accepts in physical science.

Now this raising of fundamental philosophical issues is, the writer heartily agrees, essential to psychology at the present time. It has long been the fashion for psychologists, and other scientists as well, to avoid any 'metaphysical' problems as unprofitable, if not actually dangerous, to science. That they were all the while committed, even through this very protest, to a highly questionable metaphysical dogmatism,

Köhler has very pertinently pointed out. But if Köhler is justified, as a psychologist, in raising the epistemological issue, his manner of settling it is far from satisfactory either to science or to philosophy. His own position is committed to all the dangers and difficulties of 'bifurcation,' to use Whitehead's term.

All scientific knowledge, Köhler contends, rests upon direct experience and upon the acceptance of an 'outer' world of things and people and events, which is somehow distinguished from an 'inner' world of feelings, memories, intentions, etc. But—and here the cloven foot appears—physical science does not *deal with* the outer world of direct experience. It substitutes for it *another* world of objects and events totally different from those actually observed by us. Just how this substitution occurs remains a mystery, but it is justified because in some vague way it 'works.' But surely it works very badly when one comes to examine it. Let us ask first, however, why it is necessary to make the substitution at all. There are, it appears, two reasons. In the first place, science finds the world of naïve experience full of inconsistencies and contradictions. Since these are intolerable to science, it has been compelled, so Köhler assures us, to perform "the enormous and almost impossible feat of jumping out of the world of direct, but confused, experience into a world of clear and hard reality" (p. 5). The second reason is still more compelling. It is the fact of the dependence of direct experience on physiological processes within the organism. Although these processes are initiated by influences exerted by physical objects on my organism, "there is no possibility of identifying the final product, 'things' and their 'changes' as I have them in immediate experience, with those physical objects from which the influences came. If a wound is not the gun which emitted the projectile, then the 'thing' which I have before me, which I see and feel, cannot be identical with the corresponding physical object" (p. 23).

The argument against the behaviorist thus becomes clear. As the physical scientist is constrained to postulate a world of physical objects lying beyond the reach of possible experience,

so the psychologist is equally constrained to postulate the direct experience of other persons, despite its inaccessibility to his own observation. If no cogent evidence can be found for the existence of either of these 'transcendent' objects, that fact should prove no more of an obstacle to the progress of psychology than it has done to that of physics.

The first reflection that occurs on considering this argument of Köhler's is that science, as he represents it, has strained at a gnat only to swallow a camel. If it has been our intolerance of inconsistency and contradiction which has led us as scientists to abandon the position of naïve common sense, how can we possibly rest content with the situation of science as thus revealed? The only answer which is suggested by Köhler is that somehow these inconsistent beliefs of the scientist 'work.' But so do the inconsistent beliefs of naïve common sense—so long as we are concerned only with short-sighted practical procedure and have no interest in theoretical considerations. Science, however, is born of interest in theoretical considerations. There are, to be sure, theoretical considerations which may be ignored by a special science because they are irrelevant to its own problems. But the theoretical questions which Köhler has raised he can scarcely allege to be irrelevant to psychological theory. He has himself felt obliged to devote pages to the discussion of them, and to adopt a definite epistemological position. The inherent difficulties of this position, then, he cannot ignore on the supposition that they will remain without effect upon his scientific procedure and results.

Köhler is undoubtedly upon firm ground in his insistence that science rests upon direct experience. By this he means not only that it rests upon observation—upon what we ourselves directly see and hear and feel and what others also report that they see and hear and feel—but that it rests at the same time upon the naïve realism of common sense. 'Direct experience,' as understood by Köhler, is no affair of ultimate sense data which in themselves are neither 'inner' nor 'outer,' neither 'subjective' nor 'objective.' On the contrary, we directly experience 'outer' things and events as somehow

distinguished from, and independent of, 'inner' feelings and sensations. And this distinction inherent in our direct experience science must likewise accept, else it could not take the first step. So far, the present writer would cordially agree, Köhler is on firm ground. The galvanometer I use as a physicist I see directly as an object 'out there,' with a needle which moves independently of me. Moreover, I must suppose you see this galvanometer too, and observe the same readings that I do. All this is indispensable to the scientist. And yet Köhler tells us that the galvanometer *as a physical object* is a different object from the galvanometer I observe as a physicist. Not only that, but the one you observe is distinct from both. The physicist, as a physicist, is thus forced to accept the existence of three different and distinct galvanometers (p. 29). Now of course we all know what has led Köhler to this paradoxical conclusion. We see the galvanometer and all other objects as colored, for example, but this color does not belong to the objects as one of their constitutive properties to be studied by physical science. So also we observe the galvanometer as hard and solid, but these properties we may also deny to the physical object as such. The physical object is a swarm of electrons moving at relatively immense distances from one another, and so 'as such' is neither hard nor solid. But surely these, and other familiar facts of the same kind, do not compel us to conclude with Köhler—or even excuse us for concluding—that the colored, hard, solid galvanometer which we observe is one object, and the swarm of electrons which compose it is a different object. To be sure, in one sense we do not see 'a swarm of electrons,' but neither do we see 'a flock of sheep' on the distant hillside where they move as a shifting white patch. But the shepherd nevertheless sees the moving patch as his flock, and feels no difficulty in identifying the flock his dog later on brings in with the white patch he saw. Surely the physicist need feel no greater hesitation in identifying the colored, hard, solid galvanometer he sees with the swarm of electrons. Nor does he, I venture to assert, except in moments of epistemological confusion, which inevitably result from raising the question as to the nature of

the object 'as such.' The hardness and solidity really belong to the galvanometer as its properties, even though it be a swarm of electrons, if we add the proviso: *relatively to a given context.* And so also, it may be added, does its color. The acceptance of the theory of relativity does not compel us to deny that two events are really simultaneous, because they are not simultaneous 'as such,' but only relatively to a given 'frame of reference.' And similarly the acceptance of scientific knowledge in general does not compel us to take the fatal step of denying the 'reality' of the properties and objects of direct experience. The development of science involves, of course, a correction of the common sense from which it springs. The qualities and properties which we naïvely take to be absolute characteristics of things, science discovers to be relative to what we may call 'platforms of perception.' But we cannot without intellectual suicide deny to these qualities and properties of direct experience, relative though they be, a place in the larger order of nature which science progressively discovers.

The compelling reason for the sharp dualism of the world of direct experience and that of science is not, however, to be found in the development of physical science taken by itself. What makes the dualism inevitable to Köhler's thought is the dependence of direct experience upon physiological processes. "If the wound is not the gun which emitted the projectile, then the 'thing' which I have before me, which I see and feel, cannot be identical with the corresponding physical object," we found him saying. But Köhler fails to push the argument from physiological dependence to its logical conclusion. It is not only my direct experience which is dependent on physiological processes, but my indirect experience as well, including those very inferences of my scientific thought by which the world of science is attained. To be sure, no one knows very much about these physiological processes in detail, but Köhler would be the last to deny their existence or the dependence of indirect experience upon them. Moreover, they must, by his own showing, grow out of the processes underlying direct experience. Why then does he fail to admit the logical conse-

quence of this admitted relationship? If the galvanometer the scientist observes cannot be the 'physical' galvanometer since "the object of his observation is the result of certain organic processes, determined by the physical galvanometer" (p. 29), surely the galvanometer he conceives as a scientist must be even more removed from the physical object, since it is the result of further organic processes. Once accept the doctrine that direct experience is the 'outcome' or 'result' of physiological processes, and there is no logical stopping-place short of complete subjectivism. That Köhler refuses to 'follow the argument whithersoever it leads' is evidently because he sees so clearly the vicious circle to which he would then be committed—that vicious circle which thinkers have striven so desperately to avoid ever since the days of Locke. For it is only by the acceptance of a thoroughgoing realism as a premise that we are led to reach subjectivism as a conclusion; and if we admit the conclusion of subjectivism, we must thereby deny the validity of the realistic premise from which we started.

Unless we are prepared to resign ourselves to a complete scepticism of human knowledge, we must suppose that something is wrong in the reasoning that commits us to this vicious circle. Natural science, psychology included, must rest, as Köhler justly insists, on direct experience and the simple realism of common sense. It must suppose that we can and do observe a world of things and events which undergo changes unaffected by the processes of our observation. If science is forced constantly to modify this assumption as it applies to particular cases, and to correct observations which it had previously accepted, this can never lead to a denial of the assumption itself, or to the abandonment of all observation as misleading. Indeed it can correct one set of observations only by relying on others, and discover the need of modifying the assumption as it applies to particular cases only by continuing to accept it elsewhere. It is evidently not the premise of our vicious circle which is to be condemned, but some later step in the argument. Now the step of the argument we may well question is the statement that the things and events of

direct experience are the 'products' of physiological processes within the organism. It is doubtless true, for example, that I would not observe a galvanometer, if certain physiological processes did not occur within my body; but it does not follow —in fact it is highly questionable—that the galvanometer I observe is itself the 'final product' of those processes. Köhler has urged very convincingly the great need in present-day psychology of a new theoretical outlook—of productive hypotheses freed from the domination of eighteenth century thought. Surely this part of our philosophical heritage, which Köhler accepts so unquestioningly, stands in as urgent need of critical examination as does the machine theory or the Humian atomism.

2. Köhler has condemned behaviorism both because of its adherence to machine theory—which it shares with current introspectionism—and because of its refusal to deal with direct experience. The present writer has on other occasions professed adherence to a behaviorism, but she holds no brief for the doctrines attacked in its name in *Gestalt Psychology*. The term behaviorism has apparently become applicable only to that doctrine which refuses to admit any scientific value whatsoever to introspection in any form, and which holds that human behavior in all its organization has been built up from the conditioned reflex. There is, however, another theoretical position, differing sharply from introspectionism as commonly understood, and from behaviorism as just described, which is not considered by Köhler, although it has much in common with *Gestalt* theory. According to this position, psychology is not concerned with 'consciousness' or 'experience' as something ontologically distinct from the functioning of the organism as it is observable by natural science in its relations to the world about it. A feeling or thought is not an 'inner' reality essentially hidden from all but the subject, the relations of which to the 'outer' public reality are merely external. On the contrary, its relations to the objective world are essential and constitutive. Our innermost feelings and experiences can be known even by ourselves, *i.e.*, identified and recognized, only through the

relations they have to the objective order common to ourselves and others. If I am able to tell you that I 'feel cold,' that is because I recognize this feeling as one which makes me shiver and shrink from exposure, and to find an extra wrap or approach to the fire comfortable. The writer once heard a three year old boy complain after eating raspberries for the first time—a food that his mother had never been able to tolerate—that he 'felt cold.' But presently our suspicions as to the real nature of his feelings were justified by the characteristic overt behavior of nausea. The little boy thus learned to distinguish and recognize a new experience as 'feeling sick at one's stomach.' It is not otherwise that the psychologist learns to introspect. It is doubtless true that the child's nausea was a 'private' experience not shared by us, the outsiders; and it is also true that while nausea is not apt to remain 'hidden' from an acute observer, the subject is often able to assure us of its existence when we should otherwise be in doubt. Introspection, then, has its place not only in common life but in psychological science. But—and this is the essential point—it is not an independent method. It can have no scientific value except as used in the closest connection with the methods of natural science. At most it is only a supplement, convenient or even indispensable, to the observation of behavior.

Koffka in 'The Growth of the Mind' has virtually admitted this, although his argument does not seem to me altogether clear and consistent. He urges against behaviorism that psychology must use 'descriptive' concepts as well as 'functional' concepts. By 'descriptive' concepts, or terms, he means those which are applicable to 'experience' or 'phenomena,' 'facts which can be established only by a single person'; while 'functional' concepts, or terms, are those applicable to '*actual* or *real* things or processes,' or 'facts which anyone can determine' (p. 8). So far, then, he seems to speak in terms of the traditional dualism, and to accept the distinction between 'inner' experience and 'outer' reality as ultimate. Later, however, he shows, by reference to various specific experiments, that descriptive concepts and functional concepts are by no means independent of each other. Thus,

for example, in the investigation of auditory perception, where the emphasis is upon descriptive (introspective) procedure, the subject reports in descriptive terms what he experiences as the experimenter varies the auditory stimuli. The report of the subject in this case is, to be sure, 'only a convenient and abbreviated type of behavior' for which training-tests for sensory discrimination may be substituted. Yet such substituted training-test, taken by itself, is psychologically insufficient and cannot really take the place of the introspective report. "For instance, suppose I test two observers, A and B, with the tones of 500 and 600 v.d., and suppose that the test has in each case been successfully administered. If we examine the reports of A and B, A may say 'the two test-tones constituted a minor third, and I reacted to the higher,' while B may express himself quite differently; perhaps he does not know what a minor third is, nor when one tone is *higher* than another. Instead B may describe his experience by saying that one tone was *duller* and the other *brighter*, and that he reacted to the *brighter* tone. Although the training was successful, and the objective behavior the same in both cases, yet so different are the descriptions given by these two observers that we must conclude the results of the training involve different types of behavior. In fact, tests are certain to show that observer A is much more capable of auditory training than observer B; yet, without knowing anything of their respective experiences, how could one find out wherein this difference lay, or upon what it depended?" (p. 27).

The descriptive concepts are psychologically important, then, not because of their own independent intrinsic value, but because of their systematic connection with functional concepts. Indeed a descriptive concept which does not lend itself to interpretation and testing in further functional concepts is psychologically useless. "Functional adequacy," Koffka writes, "always determines whether a new descriptive concept finds acceptance or rejection" (p. 18). And again, "Thus, sooner or later, inadequate descriptive concepts act as a check upon investigation; but progress can never be made, even with the aid of this check, by such a total abandonment

of descriptive concepts as the behaviorist proposed. Progress can and will come, however, with a continuous refinement of these concepts, as they are employed by investigators with constant reference to the overt response in connection with which the experience occurs" (p. 28). Further he writes: "The final outcome of the psycho-physical method [which makes use of both introspection and the observation of behavior] is a law expressed in terms of a functional concept. But this outcome is not arrived at without the employment of descriptive concepts; and under certain circumstances the definition [in functional terms] of a new descriptive concept may, indeed, be the most important result of a psycho-physical investigation." The "purely psychological method" (which depends solely on introspection), he concludes, "is not to be rejected, because it may be very useful as a beginning, or as a preparation for a scientific investigation, and it may even set new problems, and suggest new hypotheses, as well as lead to the formation of new descriptive concepts" (p. 30).

But surely to admit all this is virtually to abandon the dualism of 'phenomena' and 'reality,' between 'facts which can be established only by a single person' and 'facts which anyone can determine.' Koffka, like Köhler, is so concerned to maintain against a narrow behaviorism that introspection has a real place in psychological science, that he falls short of recognizing the full theoretical significance of the methodological facts he himself cites. Once change the emphasis of one's interpretation of these facts, and the conclusion which stands out is not that descriptive concepts are indispensable, but that *descriptive concepts have no independent scientific value*, that *their value depends on their being determined with reference to functional concepts*. Now this conclusion constitutes precisely the central thesis of what the present writer has advocated in the name of behaviorism. In view of current usage the term has doubtless become a misnomer. Nevertheless, the acceptance of this conclusion—and I think *Gestalt* theory does in truth accept it—carries with it far-reaching theoretical consequences which the *Gestalt* theorists themselves refuse to recognize. If introspection has scientific value only in so far

as its results are determined with reference to objective observations, then there can be no dualism, so far at least as psychology is concerned, between 'experience' and 'reality.' Psychology can study and 'know' experience only in so far as it stands in systematic interconnection with objective reality —forms, indeed, one systematic order with it. The dualism professed by both Köhler and Koffka seems to the writer to be a mere vestigial inheritance, having no proper function in the real economy of *Gestalttheorie*.

There is, to be sure, a distinction to be made in the course of psychological procedure, between direct experience, or 'phenomena,' and so-called 'objective reality' as known to science. In the investigation of perception, for example, this distinction appears as the difference between the description of the objective conditions of the experiment, including the stimulus, given by the experimenter, and the report of what he sees or hears (experiences), given by the subject. The problem of the psychologist is to discover the determinate relation between the two, the existence of which psychology postulates. Not any description of the objective conditions of the experiment will prove relevant, and his task is to describe and analyze the situation in relevant terms. To do this he may have to vary what may be called the 'physical' conditions of the experiment in order to determine inductively what its essential features are (often a difficult problem), and he must be guided throughout in his procedure by the relation between the objective situation and the subject with his capacities for behavior. We have already seen that the descriptive terms of the subject's introspective report must be interpreted with reference to his behavior, actual and potential. While there is, then, a distinction between the 'experience' of the subject in a given situation and the situation as it exists 'objectively' for science, the two belong to a common system, the structure of which it is the task of psychology to determine.

3. One of the most important features of *Gestalttheorie* is the conception of animal and human behavior as containing characteristic configurations. While Köhler, in particular,

has been most interested in demonstrating and investigating the presence of *Gestalten* in the field of sensory perception, the doctrine of *Gestalt* applies more widely. It is, in fact, highly significant that configurations are found to exist not merely in the subjective content of experience, but in the objectively observable behavior of living beings. Neither human nor animal behavior is resolvable into a mechanics of muscle and limb. It exhibits characteristic configurations both of 'attitude' and of 'act,' and these it is the business of psychological science to discover and to analyze in their systematic interrelationships. Scientific analysis, of course, could never discover these configurations if it did not find a relevant point of departure in common sense. But, as the *Gestalt* theorists have abundantly shown, we constantly distinguish such configurations in a rough and ready way, in our common intercourse with one another. The little child, for example, perceives and responds to a 'smiling face' before it has learned to see the curves of the lip and cheek which compose it geometrically. Even a dog recognizes the 'excitement' or 'determination' of his master. We perceive directly also that a cat is 'watching' the bird outside the window, that the child is 'intent' on his lessons, etc. It is from such commonsense observations as these that psychological science must start; and it is such distinctions as these that it must clarify and refine by scientific procedure. Köhler's experiments with chimpanzees will become scientific classics, not because of what they have taught us about the intelligence of apes, but because they are a conscious and deliberate attempt to carry out this sort of analysis. They spring from a new and significant theoretical insight, and they contain that most precious of scientific schievements, the formulation of new and fruitful problems.

How admirable and how opportune is Köhler's criticism of the haste of present-day 'researchers' to measure and to compile statistical tables, as if the mere attainment of quantitative results constituted science! To discern what is worth counting, to discover what is properly measurable, to formulate a 'good' problem—are not these the very life of science?

Köhler diagnoses this glorification of quantitative procedure, which vitiates so much of current research, as a symptom of the scientific adolescence of psychology. The young science apes the grown-up manners of its elder sister, physical science, without having attained the maturity to which alone such manners are suitable.

But we should recognize that there is another and very specific cause for this display of immaturity on the part of psychology; namely, the philosophical dualism which has been our heritage from the seventeenth century. Psychology has shared this common creed, which not only holds 'mind' to be a reality apart from 'matter,' but regards the entire objective world as equally 'physical.' The only conceivable alternative to a 'mechanism' of behavior has been a 'vitalism' which supposes a soul, or entelechy, to control the mechanical processes of the organism. Even when this philosophical creed is openly disavowed, it leaves its mark upon our habits of thought and influences our scientific outlook. Behavior is objectively observable—hence, we argue, it is a 'physical' reality. Why, then, look for anything but mechanically determined movements, or imagine it to be scientifically describable otherwise than in the accepted terms of physics? It is this theoretical predisposition which blinds us as scientists to the forms and figures which the pattern of living behavior displays. Once free our minds and clear our sight, and we may recognize that such 'subjective' states as anger, despair, or determination are as closely and essentially related to the directly observable behavior of human beings as is physical strain or momentum to the observable behavior of stones and sticks. Science must discover for the one set of factors, as for the other, indirect tests and methods of measurement. But the first is no more essentially 'hidden' than the second; nor is the second, more truly than the first, constitutive of a 'real' order which science substitutes for the observable order of 'phenomena.'

Now this view of the situation of psychological science has much in common with that expressed by Köhler in 'Gestalt Psychology.' Yet there are important divergences.

To the present writer, Köhler seems to fail to push his arguments to their logical conclusion and to draw the full theoretical consequences which are implied in his own positive procedure. It would be hard to exaggerate the importance of the attack on the so-called 'constancy hypothesis.' The supposition that definite elements of experience are correlated with the discharge of particular groups of cells in the nervous system, and the whole conception of experience as somehow a complex of elementary processes, has exerted a baleful influence on psychology down to our own day. The contention of the *Gestalt* theorists, that there is no such one-to-one correspondence even in the sensory field, is surely sound. That physiological processes in the cortex serve to explain the content of experience only in so far as these are treated as phases or moments in a total system—this doctrine has been advanced from many quarters.[1] But it still remains to apply it thoroughly and systematically. *Gestalt* psychology is making a serious attempt to do this, and no one can deny, I think, the great fruitfulness of the enterprise or the great value of the results it has already attained. How essential to this larger enterprise the specific 'dynamical theory' may be, which Köhler would substitute for the outgrown 'machine theory' of nervous action, still remains to be seen. The present writer for one is content to maintain on open mind on that question. But, however valuable the dynamical theory of nervous action may prove to be in itself, its association with psycho-physical parallelism raises grave difficulties.

Is there a 'brain field,' the dynamic condition of which, if it could be directly studied, would be found to 'picture' in representative fashion the total content of experience as an interrelated whole of objects, events, and feelings? Perhaps. But if we boldly make this assumption of a complete dynamic parallelism, how does it actually help us to understand experience? The truth is that in itself such an assumption is perfectly sterile. The interpretation of parallelism as dy-

[1] Cf. two articles by the present writer, 'Sensation and perception,' *J. Phil., Psychol. & Sci. Meth.*, 1916, 13, 533–548 and 617–631; also 'The empirical correlation of mental and bodily phenomena,' same *Journal*, 1918, 15, 533–541.

namical rather than geometrical, for example, does not essentially change it as a methodological hypothesis. We are perhaps able to imagine a parallelism between experienced contents and a dynamical field in more plausible detail. Tyndall, bound within the limits of a geometrical parallelism, was forced to suggest a right-handed spiral motion in the cortex as the correlate of 'love,' and a left-handed one of 'hate.' Speculating in terms of a dynamic brain field, we may picture our love for a friend and our hate for an enemy as correlated with antagonistic stresses between the 'part' of the field representing the experienced self, on the one hand, and that 'part' of the field representing the friend or the enemy, on the other hand. But if this (despite its dubious 'localization') is less fantastic than Tyndall's half facetious suggestion, it betrays the same fundamental type of methodological theorizing, and is open to the same type of objections. It is not necessary to rehearse all these difficulties here. The fundamental weakness of the assumption lies in this: if we suppose that the entire structure of experience is completely mirrored in the physiological condition of the nervous system, such a duplication can in itself explain nothing. The features which we are led to attribute to the unknown brain field, we have already found in experience, and their assumed physiological counterpart, even if we could establish its existence, could add nothing to what we already know of experience.

Will it be urged that this criticism of the formal methodological sterility of the hypothesis is irrelevant as applied to the position actually taken by Köhler in the volume under consideration? Köhler's argument as set forth runs somewhat as follows. Behaviorism has failed conspicuously to explain human behavior, owing, first, to its neglect of direct experience, and, secondly, to the barrenness of the machine theory it adopts as a means of explaining the actually observed characteristics of behavior. In order to make scientific progress in the explanation of behavior, psychology needs 'productive' hypotheses as to the nature of physiological processes within the organism. We can observe the physical stimuli acting on the organism from without, and also the

overt behavior; but the inner physiological processes are *terra incognita*. Yet, since behavior is determined by these unobservable processes, we must formulate such theories as we can about their nature, testing and modifying these constantly by application. Our direct experience can give us an invaluable clue as to the form and structure of these processes, provided we assume the fundamental psycho-physical principle enunciated by Hering, of the congruence, or isomorphism, in their systematic properties. Thus when I give an introspective report, "*if, to me, my language is an adequate 'symbol' for my own direct experience, it is an objective symbol for those physiological processes at the same time*" (p. 67, Köhler's italics). The hypothesis of dynamical parallelism is avowedly assumed, then, as a step in the explanation of *behavior*. Direct experience itself can have no influence on behavior, since, in Köhler's view, it belongs to a different order of reality; but if it corresponds in its structural organization to physiological processes in the nervous system, the corresponding order of these may in particular cases be inferred and used as a hypothetical explanation of the resultant behavior.

Now this is a perfectly possible program of investigation. But if we ask how far Köhler has carried it out, how he has used his dynamical parallelism to throw light on problems of behavior, the answer is a disappointing one. It is as if in his actual use of the hypothesis he lost sight of its methodological purpose, and were content merely to suggest how particular types of organization found in direct experience could be conceived to be represented in the hypothetical dynamics of the brain field. How these various suggestions as to the dynamical counterpart of direct experience are related to observable behavior, he scarcely discusses at all. Yet, apart from such applications, the suggested parallels, however ingenious, must remain sheer speculations, having in themselves no scientific value whatever.

I do not intend to imply that the dynamical theory of nervous action itself, as opposed to the so-called machine theory, is not applied by Köhler and other members of the *Gestalt* school to the explanation of behavior. That would

be far from the truth. The dynamical conception of 'closure,' for example, is fundamental for their treatment of behavior, and is applied concretely and fruitfully in other works. But these applications of dynamical theory are not dependent on the hypothesis of parallelism. Moreover, and this is our present concern, the suggestions in the work under consideration as to the dynamical counterparts of direct experience are treated for the most part with no reference to their bearing on behavior.

Sensory organization of the visual field is, for example, a topic to which Köhler devotes much attention. His central contention—that organization is a primary fact of nervous action and that unless we suppose it to exist *ab ovo* in the individual we cannot account for its development through learning or acquisition of 'meaning'—may be readily admitted. Nor need we challenge his contention that sensory organization is not to be explained in terms of an antecedent motor organization. Sensory organization and motor organization belong together as correlative phases of a total functional system. But it is just this fact which the particular physiological suggestions he offers fail to take account of. Visual organization is said to be conditioned by the dynamical configuration of a localized brain field which 'pictures' or represents it. But how, in any specific case, this assumed dynamical configuration within the sensory brain field actually functions in the control of the organism, no attempt is made to explain. Let us consider a single example. The experiences we have of things and events come to us by different senses; yet these different sensory experiences are all localized by us as belonging to a common space. The crackling of the fire which I hear, the warmth which I feel, the smoke odor which I smell, are all perceived as coming from the glowing fire which I see. Auditory, tactual, and olfactory experiences appear, as Köhler says, as localized more or less with reference to visual experiences. Now this common localization might have been built up by learning in the course of individual experience; but it might, he suggests, be explained nativistically as similar to the case of binocular vision. "In phylo-

genesis, and also in early individual life, for dynamical reasons which we do not have to discuss here, the distribution of sensory processes in the brain may have developed in such a direction that, now, all of them occur in one psychophysical field, and that, therefore, corresponding to their dynamo-geometrical relations, all sensory experience appears in one experienced space" (p. 232). But how does the occurrence of different sensory processes in one psychophysical brain field explain our perception of sights and sounds and smells in one space; or how do the dynamo-geometrical relations between the sensory processes in this brain field explain the particular spatial distribution of the objects and events we perceive? Only in so far as this cortical distribution accounts for the direction of behavior. It is the community of *function* of these different sensory processes in the control of the responsive behavior of the organism that is the fundamental fact. The look, the sound, and the smell of the fire all stimulate me to action directed to a common spot—the fireplace. The distance-receptors have developed phylogenetically as supplements to contact-receptors, and they function in the closest relationship with them. Yet this community of function, through which stimuli from eye and ear and hand orient the organism toward the common source of stimulation, is entirely ignored in Köhler's account. It may be, of course, that this community of function is physiologically mediated by the occurrence of the different sensory processes in a single psychophysical field, but the whole problem is to show that this is the case. And this problem Köhler does not touch.

A point which Köhler is anxious to make against a simplified behaviorism is that sensory organization is 'given' and is not dependent on attitude. It is, of course, incontestable that the total organization of the perceptual field is determined with reference to the general attitude or 'set' of the organism. Köhler would not, I understand, deny this. Indeed his own experiments with chimpanzees show repeatedly how the set of the hungry animal toward the food goal determines the organization of its perceptual field. And his whole account of the solution of problems with 'insight' goes to show that

the organizing of the perceptual field and the organizing of the response are phases of one and the same total process. Nevertheless, there are forms of sensory organization, visual wholes, for example, which are relatively independent of changes of attitude, and these Köhler treats as due to purely dynamical factors of the nervous processes of the sensory field considered merely by itself. Now one does not need to suppose that for every 'sensory whole' there is a specific response. That is obviously untrue to the commonest facts. But one must suppose that the ability to see any sensory whole is correlative to the capacity for a corresponding type of organization in behavior. And both these correlated capacities are determined in the last resort by the structure of the nervous system. The distribution of nervous energy need not, of course, be determined by definite pathways, as the traditional machine theory has taught, but it does occur within a complex and variable structure, which must, to an unknown degree, determine the organization of the dynamic field. Furthermore, it is a fundamental and peculiar characteristic of the living organism that the structure of the nervous system is progressively modifiable by nervous functioning. The whole problem of learning is, of course, set by this fact. Köhler talks like any mechanist of the dependence of reproduction on 'traces,' but he gives no least hint regarding the physiological basis of these, and the problem remains how they are to be incorporated in his dynamical theory. And the same thing is to be said regarding the assumed congenital capacity for perceiving configurations. How are we to construe the facts of mental heredity?

That the dynamical theory can be applied to the explanation of perceptual configurations only by means of reference to attitude and behavior is evident again and again in the work of Köhler and others of the school. In the experiments with chimpanzees, as we have just remarked, the configurations of the visual field were determined with reference to the 'goal.' The hungry animal was 'set' upon the attainment of the banana or other tempting food, and consequently what he saw was perceived in its relation to this goal. Nor was this

relation one of simple spatial proximity to the goal, although this was an important factor. It involved the potential 'usability' or 'effectiveness' of the nearby stick or blanket as a means of attaining the goal, and this effectiveness was relative to the chimpanzee's modes of behavior. The dynamics of the underlying nervous processes taking place in the chimpanzee, which we assume to correspond to the visual configuration of goal, stick, etc., must accordingly have been determined throughout by the chimpanzee's organized readiness for response. The particular response made in a given case depends, of course, on the particular constellation of stimuli. But the perceptual organization is created by the organism along with the organization of response. Nor does it matter in this connection how far the modes of response which are functionally effective in such a case are learned in the course of experience; however acquired, they are part and parcel of the chimpanzee's present functional equipment.

The same relationship of organized capacity for response to perceptual organization holds generally. Koffka points out, for example, that it is not the simplest forms, geometrically considered, that are earliest discriminated and recognized by the child; but those which are biologically the most important ('The Growth of the Mind,' p. 289). Expressions of human faces, such as 'friendliness' or 'anger,' are relatively primitive phenomena to the infant, much earlier recognized in spite of their great geometrical complexity than simple forms like the square or circle. Again, a little child "fails to recognize isolated parts of objects he would recognize without hesitation were they exhibited in their proper relations." Thus an ear, a mouth, or a finger was not recognized in Binet's tests when shown separately, although as parts of a 'person' they were easily recognized. This means, of course, that the child's congenital capacity for forming sensory configurations is determined along with his congenital capacity for biologically important modes of response. Just how the nervous processes underlying the sensory configuration of 'friendliness' or 'anger' are to be explained in terms of dynamics, we need not here inquire. But the example serves to show the in-

competence of the dynamical theory to account for the sensory phenomena except as it is used with the closest reference to behavior.

4. It is an assumption of Köhler's dynamical parallelism that experience is immediately correlated with the dynamical processes occurring in a limited brain field. It is the organization of this local field which 'pictures,' or represents, the organization of experience. We have already seen, for example, that the experienced localization of colors, odors, etc., in a single phenomenal 'space' is interpreted as being correlated with the occurrence in a single psychophysical field of the nervous processes underlying these different sensory experiences. And we shall presently see that he thinks of a certain 'part' of this field as corresponding to the experienced 'self,' and of another 'part' as corresponding to the experience of the 'objective' world, while the 'attitude' of the self to what is objective is represented by varying dynamical stresses within this limited field. Now this doctrine that experience is directly correlated with the processes occurring within a definite and limited region of the nervous system is full of pitfalls. Köhler has himself urged with great justice that it is a fallacy to suppose there is a 'retinal organization' which corresponds in any significant sense to the experienced 'visual organization' (p. 176, 195). As he rightly contends, retinal organization is psychologically insignificant, or rather what counts as psychologically significant in retinal stimulation is determined relatively to the place the processes in the stimulated end-organs have in the functional dynamical system. To say that, taken by itself, there is or is not 'retinal organization' is psychologically meaningless. But must we not say the same of the 'organization' of any localized brain field? Is not its dynamical organization, considered by itself and apart from the role the constituent processes are playing in the total functional system of the organism, also psychologically indifferent? There is, I think, a point of fundamental theoretical importance involved here.

A passage which occurs in the beginning of the chapter on *Association* is of especial significance in this connection.

Köhler is arguing that psychology can not be developed simply as a science of direct experience. "For the development of that science," he writes, "the field of actual experience alone is inadequate, especially when compared with the totality of those processes in the nervous system, a few of which are at each moment accompanied by experience, but *all* of which seem to be more or less interdependent. How can one pretend to construct an adequate theory of psychological events using experience alone, if the processes underlying experience are merely a dynamical province of a much larger functional whole? It is almost impossible to deny this fact nowadays. Therefore we cannot hope to understand experience itself exclusively from its own aspect, any more than one could hope to learn and understand the game of chess through watching only the moves in one corner of the board the whole time" (p. 269). The significant point to note in this passage is that Köhler himself uses the fact that "the processes underlying experience are merely a dynamical province of a much larger functional whole" merely to explain the felt incompleteness of experience—the experienced 'pointing-beyond' of what is directly experienced. "So experience tells us about its own functional incompleteness. Nor should we be too much astonished by the fact; nothing else can be expected if, of a larger functional whole, only a restricted field has experience as a correlate; to the dynamical dependence of this field upon definite physiological states beyond itself corresponds the felt definite direction in experience" (p. 270–1).

Now my criticism of this is that Köhler just fails to draw the full conclusion which is involved in the facts which he points out. For it is not merely the felt *incompleteness* and *direction* of experience that is to be interpreted with reference to the fact that "the processes underlying experience are merely a dynamical province of a much larger functional whole," but the *whole content* of experience in its total organization. There is no parallel whatever to be made out between the organization of experience and the organization of processes in the 'dynamical province' underlying experience, *considered by itself*. There is no reason to suppose that if we

could examine this 'province,' this 'fragment of a larger functional whole,' by itself, we should find any clue whatsoever for the interpretation of experience. What is experienced as 'complete' as well as what is experienced as 'incomplete' is determined with reference to the role the processes within the field actually play in the complete functional whole. The analogy with the game of chess is a good one, but it is, I think, to be carried further than Köhler attempts to carry it. It is not merely that the observer who could watch only the moves in one corner of the board would "become aware of the fact that something important was going on beyond the narrow field of his observation, that evidently the moves in it referred to something beyond since they seemed to come from there and to disappear again into the unseen." He would in truth, unless he knew beforehand that he was watching 'a game of chess,' see no 'moves' at all, but only movements of colored figures over colored squares. He would not see, for example, the position of the white knight as threatening the black castle, or the impending move of the black castle as putting the white king in check, but only movements of variously shaped black and white figures to form one geometrical grouping and now another. Yet these movements of colored figures are actually at the same time 'moves in a game of chess,' and the groupings are 'strategical arrangements' of 'defense' and 'offense.' Whether they are actually observed thus as 'moves in a chess game' does not depend merely upon whether or not the observer is restricted to a corner of the board, but upon whether he has the insight to see them in that larger functional whole which constitutes a game of chess. Now our aim as psychologists is to analyze and describe what is going on in the brain field, not as 'movements,' or even as 'dynamical stresses,' but rather as 'moves' in the larger game which the organism is playing in the world in which it lives. For experience depends on such 'moves' and not on the 'movements' or 'dynamical processes' which, physically speaking, bring them about. If the full implications of this distinction which I have thus suggested were worked out, they would, I think, carry us beyond both psycho-

physical parallelism and the underlying dualism of 'reality' and the 'phenomenal' world of experience.

5. In so far as the relation of attitude or directed behavior to the organization of experience is explicitly discussed by Köhler in 'Gestalt Psychology,' it is dominated by his dynamic parallelism in what the present writer finds to be a curiously confusing fashion. It is recognized that there is a constant reciprocal interplay between the 'set' of the organism (whether in hunger, sexual excitement, or absorption in a 'task') and the organization of the perceptual field. But this is interpreted as an experienced tension between the 'self' and 'objects,' or, physiologically, as a stress existing between the processes in the limited brain field underlying 'objective' experience, on the one hand, and those underlying 'subjective' experience, on the other hand. In other words, the constant reciprocity between the motor set and sensory distribution, characteristic of the total nervous system as a functional whole, is transferred bodily, as it were, to the limited 'dynamical province' underlying experience. Here it appears as a 'bipolar organization' of the processes underlying 'subjective' and 'objective' experience. Accordingly, experience itself is declared to have normally a 'bipolar organization,' in which we are aware of the 'self' as directed toward or away from 'something else.' This bipolarity characterizes practically all experience except states of drowsiness or low vitality. Now, not only does this position involve the confusion pointed out in the last paragraph, but it also rests upon a very questionable interpretation of experience. Experience doubtless does normally contain both subjective and objective factors; but does it normally have a 'bipolar organization'? Surely the 'subjective' factors of experience are not identifiable with 'experience of the self.' The child, and often the sophisticated adult, may feel hunger without attributing it to his 'self.' To feel hungry and to be aware that one is hungry are not identical experiences, and nothing is commoner than to have the one experience without the other. In fact hunger is often experienced as a general irritability and restlessness in which the objects and events about us undergo a subtle

transformation. The book we are reading becomes 'dull' perhaps, and the chair 'uncomfortable,' while odors from the kitchen, when we catch a whiff, become endowed with a peculiar and characteristic 'deliciousness.' And who has not felt the world a better place after dinner, without dreaming of attributing this change to his 'self'? And similarly if one is 'in love.' Must one experience this distracting state as a 'felt tension between the self and someone else,' in the self-conscious fashion that Köhler assumes? And is not the deepest passion that in which one becomes oblivious of self and absorbed in the object? Or take the case of being 'directed on' a task—so long as one has not really undertaken it, one may feel the task as something 'dark' and 'threatening,' exerting an unwelcome compulsion on one; but once one commits oneself to it—so that it is evident to an observer that one is intent upon it, or directed on it—the felt compulsion vanishes.

There is, indeed, a very important distinction to be recognized between the experience of 'self' and the 'subjective' factors in experience. What is truly subjective does not exist as a separate factor which is to me set over against what is objective. Not only my hunger, my fatigue, or other general physiological condition, but my desires, my prejudices, and my interests generally are experienced in terms of the qualities they induce in the persons and events about me. They create a special perspective, in which details stand out or are obscured and relationships are altered; there is a shift of values and a special 'coloration' of my world. Jealous Othello became alive to acts and attitudes of Desdemona he would otherwise have found neutral and insignificant, and both she and Cassio acquired in his eyes new qualities and traits. To become aware of one's self as the seat of an emotion, on the other hand, involves in so far—as Spinoza long ago pointed out—a liberation from its compulsion, a directing of attention to one's self and away from the disturbing source.

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THE ANALOGY BETWEEN MENTAL IMAGES AND SPARKS

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Introduction

W. McDougall [1] in a paper 'On the seat of the psycho-physical processes' argued in 1901 that the passage of the synapse is a discontinuous process comparable with sparking between the knobs of a Wimshurst machine, and that each spark generates a pulse of sensation.

That such a process may be of great social importance is suggested by the following quotation from Graham Wallas:

". . . the formation of the original hypothesis, the inventive moment on which successful action depends, must take place in an individual brain. If we wish to estimate the real possibility of using the ever-growing mass of recorded fact for the guidance of organised social action, we must think, not of the long rows of tables and microscopes in a scientific laboratory, nor of the numbered stacks of books and maps in the British Museum or the Library of Congress, but of a minister or responsible official when he has put back his books on their shelves, has said goodbye to his last expert adviser, and sits with shut eyes at his desk, hoping that if he can maintain long enough the effort of straining expectancy some new idea will come into his mind." [2]

That description suggests the electric strain in a spark-gap before the spark passes.

The long suspense ending in the sudden decision is not restricted to important people, nor even to humans. For Köhler [3] frequently observed that a hungry chimpanzee, which

[1] W. McDougall, *Brain*, 1901, 24, p. 577.

[2] Graham Wallas, The great society, Macmillan & Co., 1914, p. 17.

[3] W. Köhler, The mentality of apes, Kegan Paul, 1925, pp. 60, 111, 112, 180, 184, 200.

had tried and failed to reach the only visible food, sat down and gazed quietly about during several minutes, and then suddenly obtained the food by a different movement in a few seconds.

The author's attention was drawn again to the analogy by the following known experiment:

A commercial neon-lamp was connected across a high tension battery and the voltage was reduced by steps of about 2 volts. The lamp came thus to a voltage at which it did not light immediately but lit after the voltage had been applied for some time. In successive trials at the same voltage the suspense varied irregularly between 5 and 69 seconds. This fickle behaviour, so unusual in physical things, reminds one of the fickle behaviour of one's mind when the same problem has to be solved on the blackboard once a year for many years in succession.

Numerous researches on the neon-lamp have been published, and the object of this study is to see whether they contain anything helpful for psychology.

At the outset I wish to disclaim four ideas:

I. It is not suggested that the mind is entirely a mechanism, but rather that the mind is connected with a mechanism which we try to manage.

II. It is not suggested that a spark-gap becomes conscious when a spark passes.

III. It is not suggested that there are ionized gases in the brain.

IV. It is not suggested that the electric current in a wire is at all like the impulse in a nerve axon. The analogy to be developed has to do with synapses and not with axons.

On the contrary the working ideas of this enquiry are:

(*a*) That the time-changes of imagery in the mind are so like the time-changes of electric current in a neon-lamp when exposed to a potential difference near to its sparking potential, that if we can find an equation, which describes the behaviour of the lamp with respect to time, we should be able to find psychological names for the symbols, names which will make the equation a description of mental events. There is a

presumption that the symbol called potential-difference in the theory of the lamp must be renamed the intensity of trying to think. And that the symbol called energy-radiated may be renamed imagery. But let us hold these first ideas for revision as we go on.

(*b*) That although an attempt to fit equations of this kind directly to introspective data has already been made [4] with some success, yet we are more likely to reach the truth if we let the present analogy develop in its own way, delaying comparisons with the published equations until the present enquiry has matured.

(*c*) That there are ionized liquids in the brain.

Statement of the Analogy

The analogy is divided into parts numbered by Roman numerals. For each part, the physical property of the spark is lettered *A*, while the corresponding aspect of the mind is lettered *C*, and physiological analogues, when noticed, are lettered *B*. Any facts that seem contrary to the analogy are lettered Z, and a reason is stated for neglecting them.

I. *A*. For a given spark-gap there is a critical voltage such that lower voltages do not cause a steady spark to pass however long they may be applied.

I. *C*. For a given problem there is a critical intensity of trying-to-think below which an answer will never be attained. For the sort of problems that we are accustomed to solve in say one minute, the critical intensity may escape our notice because it is a state of drowsy inattention below our ordinary wide-awake level.[5]

I. Z. The solution of problems during inattention, has been entertainingly described by Poincaré,[6] and has been experienced by many people. Dr. Maxwell Garnett has suggested to me that this inattentive discovery is essentially connected with the functioning of two or more synapses in series. Such a series-effect, being complicated, is left over for future investigation.

[4] L. F. Richardson, Imagery, conation and cerebral conductance, *J. Gen. Psychol.*, 1929, 2, 324.

[5] See reference 4, pp. 341, 342.

[6] H. Poincaré, Science and method, Nelson & Sons, pp. 46–63.

II. *A.* For voltages slightly above the critical voltage, there is a delay or retardation (Funkenverzögerung) between the application of the voltage and the occurrence of the spark.[7]

II. *C.* The answer to a problem often occurs to us after a few seconds of vain attempt to think.

III. *A.* The electric current during the delay is usually at first very small and varying very slowly. Then it increases rapidly to the spark.

III. *C.* It is a common experience that, at the beginning of an attempt to solve a problem, no result is attained, and none seems to be arriving. A few seconds later the answer arrives rather suddenly.

IV. *A.* When a given spark-gap is subjected to a fixed voltage until a spark occurs after delay δ_1 seconds, then to no voltage during T seconds, then to the original voltage again during δ_2 seconds, after which time a second spark occurs, it is observed [8] that:

δ_2 is on the average equal to δ_1 if T is long enough, but δ_2 is on the average less than δ_1 if T is short enough.

IV. *C.* If we attempt to solve the same problem twice in succession with, between the two attempts, an interval T during which we think about other things, it is observed that the delay δ_2 at the second attempt is less than the delay δ_1, at the first attempt unless the interval T has been so long as to allow us completely to forget the answer.[9]

V. *A.* For a given initial condition of the electrodes and of the gas, the mean delay varies approximately inversely as the excess of the actual voltage above the critical voltage.[10]

V. *B.* The latent period for a reflex is shorter, the stronger the stimulus, within a middle range.[10a]

V. *C.* In emergencies and examinations when people want quick answers some try harder to think.

[7] K. Zuber, *Ann. der Physik*, 1925, **76**, p. 231; also W. Braunbek, *Zsch. f. Physik*, 1926, **36**, p. 582.

[8] W. Braunbek, *Zsch. f. Physik*, 1926, **36**, p. 583.

[9] L. F. Richardson, *op. cit.*, p. 326.

[10] W. Braunbek, *Zsch. f. Physik*, 1926, **36**, p. 604.

[10a] C. S. Sherrington, Integrative action of the nervous system, Constable, 1915, pp. 19–21.

VI. *A.* When all the conditions are macroscopically alike, the spark-delay varies at successive applications of the same voltage. Zuber and Braunbek have published statistics of observed times.[11]

VI. *C.* Mental measurement always involves a scatter which requires statistical treatment. As evidence it is sufficient to note that a well-known text book with the title 'The essentials of mental measurement' [12] consists chiefly of statistical theory and practice.

VII. *A.* A neon-lamp exposed to a steady voltage, just below its sparking potential, is occasionally observed to light up feebly and go out again of its own accord (Vorentladungen). Braunbek's statistical theory explains this.[13]

VII. *C.* Introspection reveals, during the suspense of trying to solve a problem, occasional images so fragmentary and fleeting that they can hardly be recognised.[14]

VIII. *A.* A neon-lamp, placed in parallel with a capacitance K and the combination of these two being arranged in series with a wire-resistance R and a battery B having a voltage considerably exceeding the sparking potential, is known to produce oscillations.[15] The explanation is that the lamp flashes almost as soon as the capacitance has filled up to the critical level. The lamp then drains the reservoir and goes out. No self inductance is involved. The current, though periodic, is far from sinusoidal. The pauses between flashes can be longer than the flashes themselves. Frequencies between 0.003 sec^{-1} and 95000 sec^{-1} have been obtained in this way.[16]

VIII. *C.* Automatic repetition is prominent in mental phenomena. For instance:

(i) Repeated notes and trills in music.

(ii) McDougall[17] showed that a single momentary stimulus to the retina generates a train of pulses of visual sensation of

[11] See footnote (7).

[12] By W. Brown and G. H. Thomson (Camb. Univ. Press).

[13] *Zsch. f. Physik*, 1926, **39**, p. 22.

[14] L. F. Richardson, *op. cit.*, pp. 327–329.

[15] S. O. Pearson and H. St. G. Anson, *Phys. Soc. Proc.*, 1922, **34**, p. 204. J. Taylor and W. Clarkson, *Phys. Soc. Proc.*, 1924, **36**, p. 269.

[16] U. A. Oschwald and A. G. Tarrant, *Proc. Phys. Soc.*, 1924, **36**, p. 265.

[17] W. McDougall, *Brit. J. Psychol.*, 1904–1905, **1**.

diminishing intensity, the train being long in proportion to the intensity of the stimulus.

(iii) When, in reading a novel, the last word of a page was strongly emphasized, that word recurred uninvited during the reading of the next page. The word sometimes recurred at fairly regular intervals of about six seconds. But the phenomenon differed from a damped harmonic oscillation because the pauses were longer than the disturbances and because the second recurrence was often more intense than the first or third.[18]

VIII. *B.* In the sensory nerves electric impulses have been found following one another at a rate of the order of 50 per second. Although periodic, they are far from sinusoidal, and therefore quite unlike the oscillation of a tuned circuit containing inductance and capacity. This is clearly shown in some curves computed by Adrian [19] from his own photographs. Again the pauses are often longer than the disturbances.

To complete the analogy we should have to seek in the brain for some things having differential equations with respect to time like the equations of a neon-lamp, a wire-resistance and a capacitance, but there is no need to look for anything having time-equations like those of an inductance. The synapse is strongly suggested as the part to compare with the neon-lamp. To make a rash suggestion: the cell body of a neurone looks like a capacitance.

IX. *A.* The spark-delay varies inversely as the ionization in the spark-gap due to external causes, such as X-rays, when the voltage and other circumstances are maintained constant.[20]

IX. *C.* Delay in arriving at an answer is less as the individual is cleverer, other circumstances being the same. Cleverness is here used in the sense defined by Garnett.[21]

[18] L. F. Richardson, Imagery, conation, and cerebral conductance, *J. Gen. Psychol.*, 1929, **2**, pp. 337 to 339.

[19] E. D. Adrian, The basis of sensation, Christophers, 1928, p. 52, Fig. 6D.

[20] W. Braunbek, *Zsch. f. Physik*, 1926, **36**, p. 604.

[21] J. C. M. Garnett, *Brit. J. Psychol.*, 1919, **9**, also *Proc. Roy. Soc. A.*, 1919, **96**, pp. 91 to 111.

Interim Summary

(1) The preceding comparisons suggest that we should take the symbol called voltage in the equations of the spark-gap, and rename it 'intensity of trying to think.'

(2) Also that we should apply the name 'intensity of relevant imagery' to one or other of several symbols:

(*a*) Either to the symbol for electric current;

(*b*) Or to the symbol for any physical quantity, such as the time-rate of energy, which is proportional to the current when the voltage is fixed.

(That word 'relevant' is connected with philosophic problems which are here ignored.)

(3) Also that we should apply the name 'cleverness' to the symbol for the intensity of ionization due to external causes such as X-rays, γ-rays or cosmic rays.

(4) So far I have taken known facts about the neon-lamp and have found their mental analogues. Encouraged by success, I next tried to reverse the process, taking known mental phenomena and using them to predict the results of experiments on neon-lamps. Accordingly X. *C* precedes X. *A*.

X. *C*. One of the most remarkable of mental phenomena is the extinction of a thought by its successor, a process which occurs in everyday thinking, reading or conversation. Spearman includes it in universal mental competition.[22] McDougall [23] has a very interesting and comprehensive theory of the same phenomena described as 'inhibition by drainage.' Garnett [24] has applied McDougall's theory to education, calling it one of the five laws of thought.

X. *B*. An apparently similar phenomenon is well known to physiologists as reciprocal inhibition [25] and the drainage theory is one among many put forward to explain it.

A critical survey of facts and theories of inhibition was

[22] C. Spearman, The abilities of man, Macmillan, 1926, Ch. VIII.

[23] W. McDougall, *Brain*, 1903, 26, p. 153.

[24] J. C. M. Garnett, Education and world citizenship, Camb. Press, 1921, p. 70.

[25] Sherrington, Integrative action of the nervous system, p. 201. K. Lucas, The conduction of the nervous impulse, Longmans Green, 1917, p. 94.

given by Bayliss.[26] He believed in the existence of directly inhibitory fibres or end-organs. Dr. R. D. Gillespie tells me that a corresponding direct inhibition, not caused by distraction, finds its place in psychopathology. L. B. Turner [26a] has given an electrical model showing such simple inhibition. But none of these facts is opposed to the view that there exists also another kind of inhibition, namely by distraction, as in X. *C*.

X. *A*. From the mental phenomena in X. *C* it seemed reasonable to predict:

(1) That it would be possible to connect two neon-lamps to the same battery so that the lighting of one lamp should extinguish the other; the change-over depending on the properties of the lamps, and not *merely* on the movement of a metal 2-way switch.
(2) That resistance, obeying Ohm's law, would be required in the circuit.
(3) Possibly also capacitance.
(4) But not inductance.
(5) That under some circumstances, analogous to reverie, the second lamp would start of itself.
(6) That under other circumstances, analogous to hard purposive thinking, the second lamp would require an extra voltage, analogous to the Will, to start it.

The connections were not indicated by the analogy.

Of these predictions Nos. (1), (2), (4), and (6) were fulfilled by the apparatus shown in Fig. (1). In it *P* and *Q* were two neon-lamps.

They were prepared, from commercial Osglim lamps marked 200–220 V 5 W, by cutting open the brass caps, removing the coils of wire and making soldered connections. In each lamp one electrode was a disk, the other a spiral. For probable impurities such as hydrogen in the neon, see J. W. Ryde.[27]

The wire-coils *R* and *S* were each of 1.50×10^4 ohms, and

[26] Sir W. M. Bayliss, Principles of general physiology, Longmans Green, 1918, Ch. XIII.

[26a] L. B. Turner, Outline of wireless, Camb. Univ. Press, 1921, p. 92.

[27] J. W. Ryde, *Proc. Phys. Soc.*, 1924, **36**, pp. 278–279.

U, also of wire, had 9.74×10^4 ohms. There was a main battery, of voltage V, which could be applied steadily by putting in the wander-plug K. An extra battery of voltage W could be applied temporarily by a tapping contact either at the point x or at the point y. The experiments were made in a room lit by tungsten lamps and free from daylight or radioactive matter. The behaviour of the apparatus depended greatly on the voltage V.

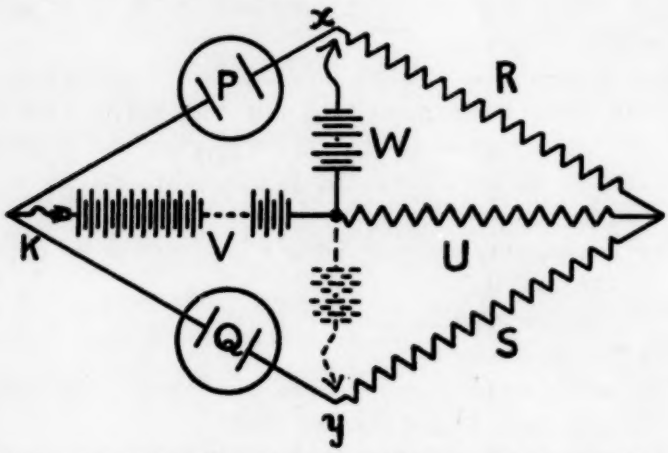

FIG. 1. Electrical Model illustrating a Mind having a Will but capable of only Two Ideas. See Analogies X., XI., XII., XIII.

$V = 154$ Volts. The lamps having been rested overnight, insertion of the plug K caused lamp Q to light while P remained extinct. Contact at x, applying the extra W of 12 volts for about a second, caused both lamps to flash, P more brightly than Q; and when contact at x was withdrawn, P remained alight but Q extinct, although the main voltage V was applied steadily all the time. Thus the lighting of P had inhibited Q. Again a temporary contact for about a second at y left Q alight but P extinct. And so on to and fro many times in succession.

It may be objected that something like a switch is used to apply the extra voltage W first at x, then at y. The reply is that W merely starts the inhibition, which is maintained by the lamps and resistances. Similarly we often have to make an effort to begin thinking about, say, a difficult theory;

but once we have well begun, the theory goes without effort, and rival ideas vanish.

McDougall [28] realized that to explain reciprocal inhibition something was required beyond mere drainage through resistances obeying Ohm's law; and he described a hydraulic model involving distended india-rubber tubes and valves controlled by springs. Now the stretched rubber is, in its equation of motion, like an electrical capacitance; and the inertia of the water is like self inductance. So McDougall's model involves two things omitted in the experiment of Fig. 1. The properties of the neon-lamp which are effective in causing reciprocal inhibition would seem to be

(1) The existence of a sparking potential. McDougall's valves had a corresponding pressure at which they opened.

(2) The fact that the sparking potential is lowered by the immediately previous passage of current and rises again while no current passes. McDougall (*loc. cit.*, p. 133, footnote) attributed exactly the opposite properties to the valves or to the synapses. He wrote ". . . each valve should tend to return to the closed position the more strongly, the longer it has been open." But indeed an Osglim lamp seems to show this opposite behaviour after longer times such as 20 minutes. See a paper by Shaxby and Evans [29] and discussion thereon.

Garnett [30] has emphasized the view that Will acts by reinforcing excitement; this is very like the function of the battery W in Fig. 1.

Besides reciprocal inhibition, the apparatus of Fig. 1, showed several other analogies for which further Roman numerals are assigned.

XI. *A*. At $V = 154$ volts only one lamp lit when the plug K was inserted. But which lamp it was, depended on their recent history. If both had been rested for several minutes then Q lit. But if the lamp P had been set going by a brief touch at x from W and left glowing for, say, a minute, then

[28] W. McDougall, Physiological psychology, J. M. Dent, 1911, p. 132.

[29] Shaxby and Evans, *Proc. Phys. Soc.*, 1922, 34, pp. 254, 278.

[30] J. C. M. Garnett, Education and world citizenship, Camb. Press, pp. 99, 101.

taking out plug K and inserting K again after a second, caused P to relight instead of Q.

XI. *C.* The glowing of Q is like an instinctive thought that predominates, unless P has been made easier by recently thinking P.

XII. *A.* On setting the main voltage to the lower value $V = 143$ volts, the insertion of K did not light either lamp. W was 12 volts as before. Temporary contact for one second at y caused Q to light but not P. Temporary contact for one second at x left both lamps extinct.

XII. *C.* This is like a drowsy person who by a momentary effort W can just think the easy thought Q. But if he turns his attention to the more difficult thought P, he falls asleep altogether.

XIII. *A.* When $V = 154$ volts, $W = 12$ volts, it was occasionally observed that a temporary contact at x or y left both lamps lit. When this happened a more prolonged contact at x or y was found to leave the lamp on the same side alight and opposite lamp extinct.

XIII. *C.* This may be compared with a mental conflict and its resolution by an effort of Will. I am indebted to Mr. J. S. Ross for this comparison.

XIV. *B.* Adrian[30a] has found that, in sensory nerves, impulses follow one another at a rate comparable with 100 per second.

XIV. *A.* To imitate this, however imperfectly, a rotating commutator Z, driven by a motor M as in Fig. 2, was arranged to interrupt the current about 130 times per second. The insulation-resistance between M and Z exceeded 10^{10} ohms. The lamps P and Q and the resistances R, S, U were the same in figure 2 as in figure 1. But the extra battery W was not used. The voltage of V was set at 179 volts by the aid of a voltmeter E. On making contact at K on fourteen occasions, the lamp P lit on 8 occasions alone, and Q lit alone on the other 6 occasions. When Q lit no further change was observed. But—and this is the interesting point—when P lit and the apparatus was left untouched, then after a

[30a] E. D. Adrian, The basis of sensation, Christophers, 1928.

few seconds *P* went out and *Q* simultaneously lighted up. This was observed on all but one of the 8 occasions on which *P* lit first. The delays, with *P* glowing, before the automatic change from *P* to *Q*, were distributed thus in seconds 1, 15, 7, 2, 3, 7.

XIV. *C.* This resembles the automatic change of imagery that occurs in *reverie*. Thus the experiment of XIV. *A* fulfils the prediction made in X. *A* (5) except that I have not obtained the automatic change with a steady voltage, but only when the interrupter was running.

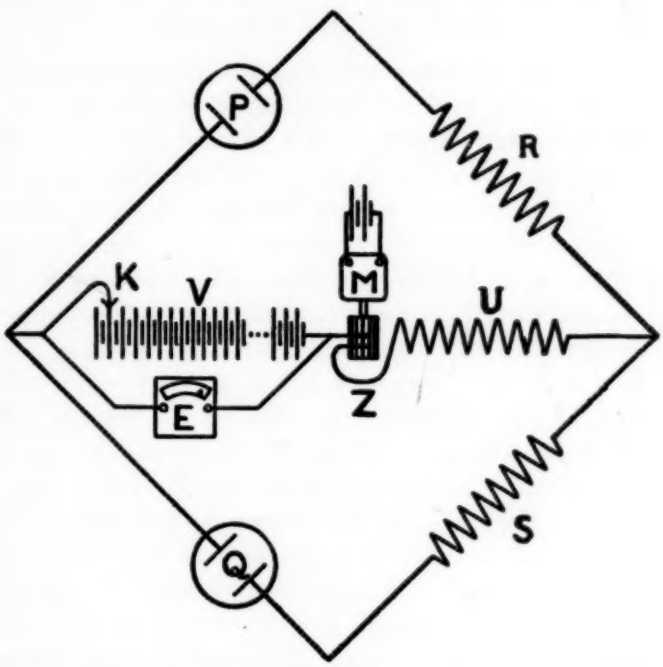

FIG. 2. Intermittent Current and Reverie. See Analogies XIV., XV.

XV. *A.* In an experiment similar to XIV. *A* it was found that the times during which *P* remained glowing before the automatic interchange from *P* to *Q* were greatly prolonged by placing about 0.0002 grm. of radium near *P* while *Q* was protected by lead screens.

XV. *C.* Ideas for which a person is clever tend to occupy his mind.

XVI. *B.* A characteristic property of the synapse is one-way conduction.[31]

XVI. *A.* Unsymmetrical spark-gaps, which have a higher sparking voltage in one direction than in the other, are well-known.[32] Experiments are in prospect with neon-lamps of this type.

XVI. *C.* Mental one-way conduction, of a complicated kind, occurs when a person, who knows the alphabet perfectly forwards, finds it difficult to repeat it backwards. But the details of this analogy are obscure.

Summary and Abstract

This is a development from an analogy between sensations and sparks published by McDougall in 'Brain' of 1901. The strain of expectation, which we feel when we wait for the solution of a problem, is here compared with the electric intensity in a spark-gap before the spark passes. The time of imageless suspense is compared with the spark-delay, noting that each of them is fickle. The least intensity with which we must attempt any problem if we are ever to solve it, is compared with the minimum sparking potential. Cleverness, in J. C. M. Garnett's sense, is compared with the ionization of the spark-gap by X-rays or γ-rays. Having found nine analogies, of which the foregoing are a sample, the author then predicted, from McDougall's theory of inhibition by drainage, that it would be possible to connect two spark-gaps so that the passage of one spark should inhibit the other; the change-over either (*a*) requiring an extra stimulus, as in purposive thinking; or (*b*) occurring automatically, as in reverie. Both predictions have been verified by using neon lamps as spark-gaps. Altogether sixteen, mutually consistent analogies have been collected. These analogies show that when physicists shall have agreed among themselves about the theory of sparking, their formulæ will be of great interest to both physiologists and psy-

[31] C. S. Sherrington, Integrative action of the nervous system, Constable, 1915, p. 14.

[32] Siegbahn, The spectroscopy of X-rays, Oxford Press, 1925, pp. 75, 76. Also Townsend, Electricity in gases, Oxford Press, 1915, pp. 378–391.

chologists. For the physical equations will provide a description of the analogous psychological events on giving the following new names to the symbols: Intensity of trying-to-think for potential difference, intensity of imagery for either electric current or energy radiated, cleverness for ionization due to X- or γ-rays.

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PLEASANT AND UNPLEASANT FEELINGS

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Feelings and Emotions.—Although the subject of feelings and emotions is quite old, the experiences denoted by these terms have generally been regarded as secondary to other more definite psychological activities. Feelings always seem to have a subjective reference, and the physiological processes involved in them are poorly understood. They are often so vague and poorly localized that they are said to lack the attribute of clearness. Some writers have claimed that a feeling is merely an attribute of a sensation, or, at best, merely a pattern of organic sensations. Others have held the view that feelings are only relations between the more clearly defined mental processes. Some writers of an older school have insisted that feelings are dependent upon cognitive processes. A few psychologists have believed that emotions are dependent upon instincts, and some behaviorists have defined emotions in terms of 'expressive' movements alone.

It is generally agreed that the feelings and emotions have been shabbily treated in the past; and it is equally clear that they deserve a better fate. Even though they are related to sensations and associated with verbal, visceral, and manual activities, they are still different from any of these. It seems justifiable to believe that a feeling or emotion includes a pattern of 'bodily' activities. Some of the elements or factors in this pattern are neurological, some are verbal, some visceral, and some introspective; but all of them are psychological, and none of them should be regarded as necessarily mysterious. If an affective activity exists, it must amount to something, and it must be the cause of one thing and the effect of something else. There are numerous relations between the

affections and other mental and bodily processes, but the causal factors operate in all directions between all of the factors in the total pattern activity. The affective experiences are not the mere attributes of other activities; they have attributes of their own. They do not appear merely as 'feeling tones' of other processes. For example, if a feeling process and a thinking process are present at the same time, there is no more justification for considering that the feeling belongs to the thinking process, and is a 'feeling tone' of the thinking, than in believing that the thinking belongs to the feeling process, and is a 'thinking tone' of the feeling. The feelings do not just occur in response to stimulation; they also stimulate other responses in the organism. They are unique processes in themselves and deserve study on their own account.

Pleasant and Unpleasant Activities.—In almost all psychologies it has been customary to compare some of the pleasant and unpleasant activities with each other. The pleasant experiences are ordinarily designated by such terms as pleasure, pleasantness, satisfier, joy, and love; and the unpleasant experiences by such words as pain, unpleasantness, annoyer, sorrow, anger, fear, and hate. The claim is ordinarily made that there is an opposition between pleasure and pain, between pleasantness and unpleasantness, satisfiers and annoyers, joy and sorrow, love and anger, etc.

These paired terms have been used in psychology partly because of the natural tendency of psychologists to employ words that are already current in every-day speech and writing. It now seems natural to think of pleasant as the 'opposite' of unpleasant, and agreeable as the 'opposite' of disagreeable. In the field of feelings and emotions, also, several theories have been advanced which were designed to further emphasize these oppositions or antagonisms. In his study of the expression of the emotions, Darwin claimed that emotions are ranged in pairs of opposites, and that in each pair there is an antagonistic action of the expressive muscles.

Modern accounts of the emotions are often preceded by a brief description of the autonomic nervous system; and it is customary to emphasize the antagonism between the cranio-

sacral and the thoracico-lumbar (or sympathetic) divisions. When one division inhibits an organ the other is said to stimulate it, where one contracts the other dilates, and if one increases the speed of a function the other decreases it.

The Alleged Opposition Between Pleasant and Unpleasant Activities is Incomplete.—It seems surprising that the inaccurate and misleading nature of these views has not been commented upon more frequently. The paired words which are current in popular speech do not correspond to the actual psychological activities. It is now admitted that Darwin placed too much emphasis on the visible movements of animals, and he did not study the emotions themselves. In the case of many feelings and emotions there are no manual or expressive movements at all which any spectator could observe or photograph. The visible 'expressions' of an emotion are merely unreliable symptoms of the emotion itself, and they are at best only a part of the whole pattern activity which *is* the emotion. Some emotions have no visible 'expression,' and some of the 'expressions' which can be observed are misleading.

The antagonism between the cranio-sacral and sympathetic divisions of the autonomic nervous system is incomplete at several points; and it remains to be shown that any feeling or any emotion is identical with the action of either one of these so-called divisions. The structure and functions of the autonomic nervous system are so poorly understood that the relations between the feelings and emotions, on the one hand, and the autonomic nervous system, on the other, are largely matters for future investigation. Some of the glands inhibit the action of other glands, but the pleasant and unpleasant feelings cannot be identified with any one of these pairs of antagonistic glandular functions. Psychologists have been unduly influenced in the past by unscientific speculations in neurology and endocrinology. The objective indicators of feelings and emotions are not effective; they are in fact either extremely poor or else entirely unreliable. The naïve form of the introspective report has always been and still is the most reliable indication of the presence or nature of an affective activity.

The Pleasant and the Unpleasant in Popular Thinking.—In popular thinking it has been customary to regard pleasant and unpleasant experiences as the 'opposites' of each other. According to the dictionary definition, the word pleasant means 'giving pleasure,' and unpleasant means 'failing to give pleasure.' These definitions convey the impression that the pleasant experiences have a more positive nature than the unpleasant ones, but the reverse is probably nearer the truth. The word agreeable is close to 'pleasurable' in meaning; but disagreeable means 'not agreeable, displeasing.' In spite of this popular usage, the unpleasant and disagreeable experiences have a clear positive quality in their own right. They are responses or activities in the body, and they do not consist merely in the absence of pleasant and agreeable activities.

The Pleasant and the Unpleasant in Introspective Psychology.—In introspective psychology it has been the rule to think of pleasantness and unpleasantness as if they were arranged on a linear scale, with unpleasantness on one side of the neutral point and pleasantness extending for an equal distance from the neutral point in the opposite direction. Since pain has often been regarded as a sensation, some have thought it desirable to devise a new word, 'unpleasure,' to be used as the opposite of pleasure. However, it may be doubted whether any two psychological activities should be regarded as the opposites of each other in a strict sense. A pleasant feeling is one mental activity, but an unpleasant feeling is a different kind of process. Pleasantness and unpleasantness are not the positive and negative aspects, respectively, of the same process. It may be seriously questioned whether the pleasant and unpleasant activities should be placed in exactly the same class or on the same psychological plane.

Complicated Relation Between Pleasant and Unpleasant Activities.—Several ancient and modern students have recognized that the relation between the pleasant and unpleasant experiences is complicated, and a list of the more stimulating and original writers on this subject should include the names of Plato, Schopenhauer, Richet, Beaunis, Hartmann, Ribot,

Marshall, Cellérier, and Pradines.[1] In his essay, 'On the sufferings of the world' (1851), Schopenhauer held that every feeling of satisfaction is negative in character, and "consists in freedom from pain, which is the positive element of existence." Cellérier states that "Le plaisir et la douleur primaires ne sont pas plus le contraire l'un de l'autre que la mort n'est le contraire de la naissance." [2]

Plato's Views.—Although Plato's remarks in the Ninth Book of the 'Republic' are sometimes forgotten, their historical significance and important bearing on the present topic justify the lengthy quotation which follows.

> Do we not assert that pain is the opposite of pleasure?
>
> Assuredly we do.
>
> And also that there is such a thing as a simultaneous absence both of pleasure and of pain?
>
> Certainly there is.
>
> In other words, you admit that there is a point midway between the two at which the mind reposes from both. Is not that your meaning?
>
> It is.
>
> Have you forgotten the language which people hold when they are ill?
>
> Give me a specimen of it.
>
> They tell us that nothing is pleasanter than health, but that, before they were ill, they had not found out its supreme pleasantness.
>
> I remember.
>
> Do you not also hear persons who are in excessive pain say, that nothing is so pleasant as relief from pain?
>
> I do.
>
> And I think you find that, on many other similar occasions, persons, when they are uneasy, extol as supremely pleasant, not positive joy, but the absence of, and repose from, uneasiness.

[1] See Plato, Philebus, between 367 and 347 B.C.; Plato, Republic, Book IX., between 385 and 362 B.C.; A. Schopenhauer, The world as will and idea, 3 ed., 1859, 3v.; C. Richet, L'homme et l'intelligence, fragments de physiologie et de psychologie, 1884, pp. 41–88; H. Beaunis, Les sensations internes, 1889, pp. 237–252; E. v. Hartmann, Philosophy of the unconscious, Vol. III., 2 ed., Tr., 1893, pp. 12–23; T. A. Ribot, Problèmes de psychologie affective, 1916, pp. 127–145; H. R. Marshall, Pain, pleasure, and aesthetics, 1894, pp. 167–298; L. Cellérier, Les éléments de la vie affective, *Rev. phil.*, 1926, 101, 426–451; M. Pradines, L'hétérogénéité fonctionnelle du plaisir et de la douleur, *Rev. phil.*, 1927, 103, 178–212, 395–420.

[2] L. Cellérier, *op. cit.*, p. 427.

True, he replied; and perhaps the reason is, that at such times this relief does become positively pleasant and delightful.

In the same way we might expect, that when a person's joy has ceased, the repose from pleasure will be painful.

Perhaps so.

Thus the repose, which we described just now as midway between pleasure and pain, must be now one, now the other.

So it would seem.

Can that, which is neither pleasure nor pain, become both?

I think not.

Again, pleasure and pain, when present in the mind, are both of them emotions, are they not?

They are.

But was not the simultaneous absence of pleasure and of pain shewn just now to indicate a state of undoubted repose, midway between the two?

It was.

Then how can it be right to regard the absence of pain as pleasant, or the absence of pleasure as painful?

It cannot be right.

Hence, the repose felt at the times we speak of is not really, but only appears to be, pleasant by the side of what is painful, and painful by the side of what is pleasant; and these representations will in no instance stand the test of comparison with veritable pleasure, because they are only a species of enchantment.

I confess that the argument points to that conclusion.

In the next place turn your eyes to pleasures which do not grow out of pains, to prevent your imagining, as perhaps at the present moment you might do, that it is a law of nature that pleasure should be a cessation of pain, and pain a cessation of pleasure.

Pray where am I to look, and what pleasures do you mean?

Among many others, I replied, you may, if you will, take as the best example for your consideration the pleasures of smell; which, without the existence of any previous uneasiness, spring up suddenly in extraordinary intensity, and when they are over, leave no pain behind.

That is quite true.

Then do not let us be persuaded that genuine pleasure consists in the release from pain, or that genuine pain consists in the release from pleasure.

No.

But it is certain that, speaking roughly, most of the so-called pleasures which reach the mind through the body, and the keenest of them, belong to this species; that is to say, they are a kind of release from pain.[3]

It is interesting that this dialogue was written as long ago as 362 B.C., because it needs such slight revision at the present time.

A Theory of the Relation Between Pleasant and Unpleasant Feelings.—The fact that pain and the unpleasant feelings seem to have a stronger positive character than pleasure and the pleasant feelings, has given rise to a theory of these experiences which has several attractive features, even if it is not scientific in every respect. According to this theory, several forms of bodily stimulation produce an unpleasant physiological tension or mental strain which is consciously or unconsciously present in all people while they are alive. This 'tension' or 'strain' may be regarded as physical, chemical, nervous, muscular, mental, or spiritual, according to one's personal taste and preference. Pain is commonly produced when this tension attains a reasonable degree of intensity, and the unpleasantness is roughly proportional to the amount of tension. Pleasure is generally present only while this tension is being reduced.[4]

If a person is hungry, for example, a strain or tension of some kind is present, and the condition is unpleasant. The unpleasantness will continue as long as the individual is hungry and as long as the tension persists; and the feelings will be pleasant only while he is eating and while the tension is being reduced. The unpleasant tension which is present when a person is hungry may continue for several days, but the pleasure from eating can only last a few minutes. Pleasure is greatest when the tension begins to be reduced, and the pleasure generally decreases thereafter, being negatively

[3] Quoted from John Llewelyn Davies' and David James Vaughan's translation of The republic of Plato, Book IX. (between 385 and 362 B.C.), first printed in 1852.

[4] Schopenhauer has discussed this theory at some length; and a criticism of his views on the negative character of pleasure may be found in E. v. Hartmann, Philosophy of the unconscious, Vol. III., 1893, 2 ed., Tr., pp. 12–23.

accelerated, until the normal tension is reached. The pleasure is roughly proportional to the rate of fall of the tension.[5] Since the degree of tension and the possibility of pleasure reach a maximum at the same time, pain and pleasure cannot be regarded as antithetical or diametrically opposed to each other. Pain and pleasure are no more on the same plane than love and hate are in the same class.[6]

Some Pleasures Have a Positive Character.—The above theory is limited by the fact that some of the pleasures, for example, in tastes, odors, and tonal intervals, have a positive character in themselves, and are definitely raised above the so-called zero- or indifference point of the feelings. On the whole, however, the theory is quite plausible and has many attractive features. The moral theologians and the students of religious matters have used it in justifying the existence of the pain and discomfort that is present in the world. They have often claimed that pain can be justified on intellectual grounds because a certain amount of pain is generally necessary before one can have the more pleasant and agreeable experiences.

Pains and Pleasures.—Although many of the differences between pains and pleasures are fairly familiar, it has not been customary to emphasize these differences in a systematic way. The pains have attracted much more attention than the pleasures, and a great deal more has been written about the unpleasant sensations, feelings, and emotions. In 1897 Ribot remarked that "The anatomical and physiological conditions of the genesis and transmission of pleasure are a *terra incognita.* In cases of physical pleasure, what takes place at the peripheral terminations, in the nerves, in the cerebro-spinal axis? Most authors do not even propound these questions. The physiology of pain, in spite of its un-

[5] In addition to pleasure, laughter may also result when the tension is suddenly reduced.

[6] This theory of the negative character of pleasure has also been summarized by P. Bousfield, in 'Pleasure and pain; a theory of the energic foundation of feeling,' 1926. Although Bousfield gives a good account of this theory, some of his other views on the unconscious and the instincts of self-preservation and death are somewhat mysterious, and will not have a universal appeal.

certainties, is rich and instructive compared with that of pleasure." [7] A similar opinion was expressed by L. Barat in 1923: "La bibliographie du plaisir physique est aussi pauvre que celle de la douleur est riche, et cette pauvreté s'explique, si l'on songe qu'il est autrement difficile de produire le plaisir en laboratoire, et surtout de le faire croître et décroître, que de provoquer et de doser la douleur. La plupart des auteurs estiment que la sensation de plaisir est, en général, plus diffuse et plus vague que la sensation de douleur et que, s'il fallait l'opposer à quelque chose, ce serait non pas à la douleur mais aux états diffus et mal localisés de gêne, de mal-être, de désagréable." [8]

Our bodies are admirably supplied with highly specialized equipment for the production of physical pain. There are more pain spots on the skin than all the other cutaneous spots combined, and a large number of nerves and tracts are said to carry impulses of pain. The sense of pain shows less adaptation and fatigue than any other sense. The transmission of impulses of pain is relatively slow, but the responses made by an animal to painful stimuli are more general and more pronounced than the responses made to any other form of stimulation. The responses to pain also have a higher survival value than any other responses. The stimulation of almost any sense will produce pain if the stimulation is sufficiently intense. The first conscious indication that we have of the existence of many of our internal organs and structures is the production of painful impulses by them. Pain is more widely distributed in the body than any other sense. If one is inclined to classify physical pain as a sensation, he will at least be ready to admit that it differs in many important respects from any other sensation.

Many physical pleasures produce fatigue and exhaustion, and sometimes become painful. When the pleasures of affection and self-love increase to a certain point, a painful element is generally introduced. In neurasthenia the threshold for pain is markedly lowered and a condition of hyperalgesia may

[7] T. A. Ribot, The psychology of the emotions, 1897, p. 50.

[8] Quoted from G. Dumas, Traité de psychologie, 1923, Vol. 1, p. 419.

result. Pain commonly causes marked irregularities in the digestion. The average person is definitely concerned over his pains if he has any, and in medical diagnosis much practical use is made of the large number of scientific facts which are available on this subject.[9]

Pains are present in about nine-tenths of all the diseases, and, according to Behan, they have been variously described as "burning, gnawing, cutting, pinching, smarting, lancinating, boring, shooting, screwing, gripping, stabbing, grinding, sharp, dull, aching, lightning, tearing, creeping, throbbing." [10] The pains described by patients may also be characteristic of certain diseases; as, for example, the "lightning pain in tabes, the gnawing pain in rheumatism, the burning pain in neuritis, the girdle pain in spinal disease, the lead-cap pressure pain in neurasthenia, the sharp, cutting pain in neuralgia, and the dull, aching pain in infectious diseases." [11] It is possible for patients to suffer from severe pains without showing any marked signs of physical deterioration; but there are other cases "in which the constant, steady and increasing pains of tubercular disease, trigeminal neuralgias, etc., have completely exhausted the patient, so that he has become thin, haggard, careworn and prematurely gray." [12] Crile has shown that painful nervous impulses may be transmitted to the brain and other organs of the body and produce important structural effects even though the subject is not conscious of the pains.[13]

The fact that there are no pleasures that are nearly as pleasurable as some pains are painful can be illustrated by Schopenhauer's suggestion to compare the respective feelings of two animals when one of them is eating the other. There is no 'sense of pleasure' in a strict sense; although there has been much romancing about the æsthetic pleasures, which have relatively little native basis, and are largely the result of

[9] See R. J. Behan, Pain; its origin, conduction, perception and diagnostic significance, 1914; also H. Beaunis, Les sensations internes, 1889, pp. 169–188.

[10] R. J. Behan, *op. cit.*, p. 108.

[11] R. J. Behan, *op. cit.*, p. 109.

[12] R. J. Behan, *op. cit.*, p. 134.

[13] G. W. Crile, The origin and nature of the emotions, 1915.

learning and association. There is little physical equipment for the production of pleasures, and what there is turns out to be relatively weak and frail. There are few pleasure spots, and few nerves and tracts which carry pleasant impulses. The ordinary pains of every-day life are stronger and more numerous than the ordinary pleasures. Some pains can persist for long periods of time, but this seldom happens with any of the pleasures. Many of the unpleasant experiences possess the attribute of clearness or distinctness to a much greater degree than has ordinarily been assumed in traditional psychology. The unpleasant experiences are for the most part clearer than the pleasant.

Some people afflicted with anhedonia take no pleasure in anything; but few cases, if any, have been reported of people who are totally analgesic and are unable to experience any pain. Pleasures carried to excess or continued too long tend to become disagreeable, but pains carried to excess or continued too long do not tend to become agreeable. If a person is so unusual as to have some intense pleasures he can be fairly certain that he will not have them long; and this somewhat melancholy condition of man has attracted the attention of many writers in the past. Robert Burton, for example, says: "For to begin at the hour of his birth, as Pliny doth elegantly describe it, 'he is born naked, and falls a whining at the very first: he is swaddled, and bound up like a prisoner, cannot help himself, and so he continues to his life's end.'" [14]

Sensations, Feelings, and Emotions.—There is considerable variation in current usage in the case of such terms as pleasure, pain, pleasantness, unpleasantness, etc. We are unable to appreciate the value of the traditional distinction between the *sensation* of pain and the *feeling* of unpleasantness. It is impossible to separate unpleasantness from pain, and no sharp line of demarcation can be drawn between pleasantness and pleasure. The tendency to classify certain psychological activities as either sensations, feelings, *or* emotions has led to much confusion in this field. It also seems impossible to draw any sharp lines of distinction between the 'physical'

[14] R. Burton, The anatomy of melancholy, 1621, 1847 ed., p. 170.

pains and pleasures, on the one hand, and the 'mental' feelings of pleasantness and unpleasantness, on the other. The physical and the mental, and the pleasant and the unpleasant, are all causally and systematically related to each other.

Other Differences Between Pleasant and Unpleasant Activities.—The unpleasant activities appear earlier than the pleasant in the evolution of the race, and also in the development of the individual. A general undifferentiated unpleasantness seems to be the only feeling or emotion that is present in the infant soon after birth, and his ability to secure pleasure is quite limited at the start. The large majority of the characteristic expressive movements in animals and most of the easily recognized facial expressions in men are related to the unpleasant feelings and emotions. The only pleasant facial expressions are smiling and laughing; but the larger number of unpleasant facial expressions include pain, grief, fear, anger, and disgust. Mantegazza held that people have more capacity for pain than for pleasure, and that they can hate more bitterly than they can love. G. Stanley Hall said that the strength of hate "is shown by the fact that while love is everywhere and always taught, and hate and anger everywhere repressed, the latter are yet so much stronger." [15] We frequently find joys definitely below, and pains far above, our expectations. In commenting on this peculiar characteristic of life, Oscar Wilde said that there are two tragedies in the world: "One is not getting what one wants, and the other is getting it."

The unpleasant experiences are more basic and central in the organization of the personality, and they are more important in the functional mental disorders, and in the morbid complexes and dreams. Practically all of the experimental studies on the physiological changes in feelings and emotions have been concerned with unpleasant feelings and emotions. The extremely disagreeable features of everyday life are more enervating than the extremely agreeable, and they more commonly cause the neuroses.

We spend more time and expend more energy in trying to

[15] G. S. Hall, A study of anger, *Amer. J. Psychol.*, 1898–99, 10, 516–591.

escape from the unpleasant situations in everyday life than in striving to attain the pleasant. According to Williams, "The impulse to relieve our own annoyances is more intense than the impulse to seek satisfaction," and he remarks further that "It is comparatively easy to interest the public in relief of need but difficult to arouse interest in improving conditions for the self-development of others." [16] The moral theologians have long ago pointed out that it is difficult if not impossible to obtain much pleasure in the world by making a direct and deliberate attempt to attain it. In the ordinary course of events it is sometimes difficult to secure pleasure by the use of any device; but if one is looking for pains and annoyances he will have little difficulty in finding them. Even when a person has attained some long sought pleasure, the pleasure is almost certain to change, and it is then some other pleasure that is desired. These psychological tendencies constitute a great leveling agency in the world, and make the mental conditions and attitudes of many different kinds of people alike in several respects.

Summary

1. Feelings and emotions are unique processes in themselves, and deserve study on their own account.

2. It has been customary to compare the pleasant and unpleasant activities with each other, but this alleged opposition is incomplete at several points.

3. Pain has a more positive character than pleasure; and the pleasant experiences frequently consist in a release from an unpleasant condition.

4. The pleasant and unpleasant experiences are on different planes, and are not the psychological opposites of each other. The unpleasant activities are more basic and central in the organization of the personality. They are stronger and more insistent, and they play a more important rôle in motivating conduct.

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[16] J. M. Williams, Principles of social psychology, as developed in a study of economic and social conflict, 1922, p. 46.

SIMPLE TRIAL-AND-ERROR LEARNING: A STUDY IN PSYCHOLOGICAL THEORY

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I

Science proceeds by a double movement. For the most part, scientific discoveries are accomplished by means of observation and experiment. Occasionally, however, it happens that a discovery is made by means of a more or less complex logical process or 'gedanken experiment.' Einstein's mathematical deduction and prediction of what may be observed in the behavior of light when it passes near the sun is perhaps as good an example of this as any.

Frequently, after the existence and characteristics of natural phenomena have been discovered empirically, it is seen that these things might very well have been deduced from facts and principles already known. When the deduction is thus performed, as a kind of afterthought, the process is more properly termed explanation. Actual prediction is more dramatic than explanation, but the two processes are logically very similar. A true deductive explanation possesses a quality of logical necessity closely akin to prediction regardless of when the empirical observation takes place. It is of the mass of such interlocking deductive explanations that scientific systems are made. In general, that science is most perfectly systematized which can show the greatest proportion of its phenomena as logically deducible from recognized principles and other known phenomena. Moreover it seems reasonable that rival systems within a science may also be evaluated on the basis of this same criterion.

It is evident that much of what passes for explanation fails of this true deductive quality. It avails little merely to

subsume a known phenomenon under some more or less general principle. It is true enough to say of any actual event that it is a case of conservation of energy, or of cause and effect. But such bare general principles of themselves alone can hardly enable one to deduce the existence and characteristics of natural phenomena. In a similar manner, the typical undergraduate behaviorist's glib explanation of the more complex forms of habit phenomena by saying of each that it is a case of stimulus and response, utterly fails of the true deductive quality. The same may be said of the fairly common, but equally futile, invocation of complexes, equilibrium, *Gestalten*, closures, *Einsicht* and the like for a similar purpose.

For an explanation to form the substance of a true system, the deduction must eventuate in some kind of genuine novelty as compared with what is contained in the original premises. This element of novelty is what was referred to above as a predictive quality in real explanation. The deductive process is a true generative activity. The known principles give rise to new knowledge as the result of a causal sequence in a high-class redintegrative organism. According to one plausible hypothesis, principles are symbolic habits which, as a result of their functional interaction within the organism possessing them, give rise to new and distinct habits. These latter constitute the new knowledge. Thus the new knowledge, while derived from the original principles, is not the principles, but something newly come into existence. By the accumulation of these bits of deductive explanation, scientific systems become enlarged very much as have systems of mathematics.

Perhaps no theorists have been more naïve in their attempts at system construction than those who seek in the principles of stimulus-response the main explanation of those forms of behavior usually called mental. It may even be that, thus far, none have failed much worse in evolving the solid substance of genuine explanation. Even so, the author has considerable confidence in the possibilities of this point of view. As a concrete example in miniature of what is believed to be a desirable direction for this movement toward systematization

to take, there is given the following account of a *simple* type of trial-and-error learning. This may be taken as a relatively uninvolved example of what has been spoken of above as a deductive explanation.

II

There appear to be a number of fairly distinct types of trial-and-error learning. The particular principles necessary to employ in their explanation, as well as the mode of combining the principles, differ somewhat according to the type of learning to be explained. Of the true trial-and-error learning, we have the relatively complex type exemplified by maze learning, where the *obvious* reinforcement of the conditioning process (or the lack of reinforcement) for the most part comes only at the end of a series or particular combination of trial acts. A strict deductive explanation of this type of learning presents special difficulties and very probably will turn out to involve some principles not needed for the explanation of the less complex types. A second and relatively simple type of trial-and-error learning is seen where each act or trial is definitely and immediately reinforced positively, if successful, or is followed by punishment (negative reinforcement) if unsuccessful. A still different, and perhaps simpler, type is where each trial act is reinforced, if successful, but is followed by no special stimulus (is merely unreinforced) if unsuccessful. It is this last type of learning *only* which we shall consider in the following paragraphs.

Numerous phenomena characteristic of this third type of learning call for explanation. These problems can perhaps best be formulated as a series of questions:

1. Why does the organism persist in its trials or attempts even after repeated failure?

2. Why, in case success does not result from its first attempts, does the organism vary its reactions, often over a very wide range?

3. What principle or mechanism limits the range of the variation of the reactions which an organism will make to any problem situation?

4. Why do organisms of the same general type sometimes differ so widely from each other in their reactions to the same (external) problem situation?

5. What principle determines the order of appearance of the several trial acts of a trial-and-error sequence?

6. Why, in the series of trial acts preceding the first success, does the organism often stupidly commit the same erroneous reaction repeatedly?

7. What constitutes success itself?

8. Why should the trial sequence come to an end as soon as success has been attained? Why should it not continue exactly as before?

9. Why, even after the successful reaction cycle has been performed one or more times, do reactions, repeatedly found to be unsuccessful, quite illogically continue sometimes to be made?

10. Why, in general, do these erroneous reactions become less and less frequent with each successful solution, and why do they at length cease altogether?

11. Why, for a particular organism, are certain trial-and-error problems so much more readily solved than are others? Why, for certain organisms, is the same problem so much more difficult of solution than for other organisms, presumably of equally good natural endowment?

12. Why, on the whole, are the trial reactions in 'blind' trial-and-error learning so much more likely to prove successful than would be a mere random sampling from the entire repertory of the organism's possible movements? Why is the organism so much more likely to try a successful act early in the trial-and-error sequence than pure random sampling might be expected to bring about?

III

Let it be assumed, at the outset, that there exist a number of unconditioned stimuli, S_x, S_y, and S_z; and that these stimuli evoke in a certain organism the responses R_x, R_y, and R_z, respectively. It is assumed, further, that these responses involve the same 'final common path' so that no two of them

can take place simultaneously. Let S_1 represent a very mild neutral stimulus evoking at the outset no observable response whatever.

Now if S_1 should accompany S_x in the same stimulus complex a number of times there will be set up the conditioned reaction tendency

$$S_1 \longrightarrow R_x.$$

In a similar manner, if S_1 accompanies S_y in another stimulus complex a number of times there will be set up the conditioned reaction tendency

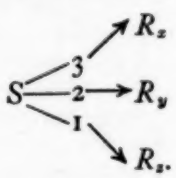
$$S_1 \longrightarrow R_y.$$

Similarly, there may also be set up the conditioned reaction tendency

$$S_1 \longrightarrow R_z.$$

Thus S_1 may come to possess a number of distinct and mutually incompatible excitatory tendencies or 'bonds.' Presumably each of these tendencies to action will have a strength or potency different from that of the others. For the sake of definiteness and simplicity of the logical consequences, we may let the strength of these excitatory tendencies stand, at this stage, in the ratio respectively of 3, 2, and 1. Lastly let it be assumed that reaction R_x is the one and only response which is biologically successful, *i.e.*, the one which is followed by reinforcement and which terminates the stimulus S_1.

Under the conditions as assumed, what might logically be expected to result in case the organism should be stimulated by S_1, either alone or in conjunction with certain other approximately neutral stimuli? It is obvious at once that there will arise a kind of competition or rivalry among the three mutually incompatible excitatory tendencies. This competition may conveniently be represented thus:



Since the excitatory tendency flowing from S_1 to R_x is strongest, this reaction will be the first trial act. By hypothesis this reaction will not be reinforced. According to the principle of experimental extinction this failure of reinforcement will weaken the tendency of S_1 to evoke R_x, leaving it, let us say, with a value of 2.1.[1] But since this excitatory tendency, even after its weakening, is stronger than either of the other two, it will still be dominant. By hypothesis, S_1 continues without interruption. Accordingly, after the brief refractory phase following the R_x response, this same reaction will be repeated as the second trial act. A second experimental extinction at once reduces the tendency to R_x to a strength of 1.2. This leaves the tendency to R_y dominant at the beginning of the third trial. S_1 continues to act. Accordingly R_y is evoked as the third trial. Here, for the first time, we note the phenomenon of variability in the trial acts.

But since R_y will not be reinforced, this excitatory tendency also will suffer extinction, reducing it to 1.1. Meanwhile, R_x has spontaneously recovered to a strength of 1.5. By hypothesis, S_1 still persists. As a result, R_x is evoked as the fourth trial of the series. Failure of reinforcement at once reduces it to the value of .6. During this time R_y has recovered to 1.4, which gives it a position of dominance. S_1 accordingly evokes this reaction for a second time, as the fifth trial act of the trial-and-error series. Failure of reinforcement reduces its excitatory potentiality to .5. Meanwhile, R_x has recovered to a strength of .9, but this is not enough to equal that of R_z, which now for the first time becomes dominant. S_1 accordingly evokes R_z as the sixth trial of the series. By hypothesis this reaction is a success and is followed by reinforcement. Since this act also terminates S_1, R_z is the last trial of the first trial-and-error sequence or behavior cycle.

[1] See Table I. for a systematic and detailed summary of the characteristic incidents of this hypothetical learning episode. No special significance should be attached to the quantitative values employed. Presumably the several types of loss and gain in strength of excitatory processes in all cases should become progressively less as the maximum effect is approached, instead of being constant as shown. The simpler constant values have been chosen with a view to simplifying the exposition. For the same reason forgetting tendencies have been neglected as well as complications involved in the acquisition of the three conditioned reactions.

TABLE I

Table showing the progressive changes that would take place in a simple case of trial-and-error learning if the influence of experimental extinction each time should be to diminish an unreinforced excitatory tendency .9 points,[2] the influence of spontaneous recovery should be to restore the loss from experimental extinction .3 points for each interval between successive trials (say one hour) and for the interval between problem cycles (say 24 hours) to restore two-thirds of the maximum diminution resulting from the experimental extinction of the preceding cycle.[3] Each successful reaction reinforces its excitatory tendency by .3 points,[4] this reinforcement being assumed to take place immediately.

Trial No.	Behavior Cycle I.		Behavior Cycle II.		Behavior Cycle III.		Behavior Cycle IV.	
	Status of Excitatory Tendencies Preceding Reaction	Resulting Reaction	Status of Excitatory Tendencies Preceding Reaction	Resulting Reaction	Status of Excitatory Tendencies Preceding Reaction	Resulting Reaction	Status of Excitatory Tendencies Preceding Reaction	Resulting Reaction
1	$x = 3.0$ $y = 2.0$ $z = 1.0$	R_z	$x = 2.2$ $y = 1.5$ $z = 1.3$	R_z	$x = 1.7$ $y = 1.2$ $z = 1.6$	R_z	$x = 1.4$ $y = 1.2$ $z = 1.9$	R_z
2	$x = 2.1$ $y = 2.0$ $z = 1.0$	R_z	$x = 1.3$ $y = 1.5$ $z = 1.3$	R_y	$x = .8$ $y = 1.2$ $z = 1.6$	R_z		
3	$x = 1.2$ $y = 2.0$ $z = 1.0$	R_y	$x = 1.6$ $y = .6$ $z = 1.3$	R_z				
4	$x = 1.5$ $y = 1.1$ $z = 1.0$	R_x	$x = .7$ $y = .9$ $z = 1.3$	R_z				
5	$x = .6$ $y = 1.4$ $z = 1.1$	R_y						
6	$x = .9$ $y = .5$ $z = 1.0$	R_x						

The second time the organism encounters the stimulus S_1 (the beginning of the second behavior cycle) the values of all the excitatory tendencies have increased over those existent at the conclusion of the previous behavior cycle. The tendency to R_z is dominant, and this reaction follows at once. The trial is unsuccessful, extinction follows, and stimulus S_1

[2] I. P. Pavlov, Conditioned reflexes, pp. 48 *ff.*

[3] Idem, *op. cit.*, p. 58.

[4] Idem, *op. cit.*, p. 40.

persists. Thereupon R_y becomes dominant, and therefore becomes the second trial act. This also is an error. Meanwhile, R_z once more has recovered to a state of dominance, and it accordingly becomes the third trial. R_z is weakened again by failure of reinforcement, which leaves dominant the correct reaction R_x. This reaction brings the second problem cycle to a successful conclusion and R_x is reinforced a second time.

The third time the stimulus S_1 is encountered it finds the three excitatory tendencies in still a different combination of strengths. R_z is dominant and becomes the first trial, an error. Its consequent weakening leaves R_x dominant. S_1 accordingly evokes R_x as the second trial. This success, as before, is followed by reinforcement.

On the fourth occasion that the organism encounters the stimulus S_1, for the first time it finds R_x dominant at the outset. Accordingly the first trial act is a success. At this point the process of trial-and-error learning may be considered as functionally complete.

IV

We may now summarize the results of our deduction by answering the questions formulated above.

1. The organism persists in its attempts because the stimulus which evokes the attempts itself persists.

2. The organism varies its reaction, when one reaction fails, because the consequent weakening of the primarily dominant excitatory tendency leaves dominant a second and distinct excitatory tendency conditioned to the same stimulus situation.

3. The range or variety of reactions which may be evoked by a given problem situation is limited to the reactions which have become conditioned during the life of the organism to one or another stimulus component of that situation.

4. Organisms superficially quite similar in general constitution may differ very widely in the nature of their trial attempts at problem solution, because their previous life history has resulted in both qualitative and quantitative

differences in their stock of excitatory tendencies evocable by the several stimulus components of the problem situation.

5. The principle which determines which of the possible trial acts shall be evoked first, second, third, etc., in the trial-and-error series is: That trial act is evoked at any given stage of the trial-and-error process which at that time is dominant, *i.e.*, strongest.

6. The reason that the organism frequently, and apparently quite stupidly, tries an unsuccessful act over and over during the first problem cycle, despite failure of reinforcement, is quite simply that the several processes which are continually varying the strengths of the different excitatory tendencies may on more than one occasion leave any particular excitatory tendency dominant. This may even result in the same erroneous act taking place two or more times in immediate succession, as in trials 1 and 2 of Behavior cycle I. (See Table I.)

7. Not enough is yet known concerning the psychology of learning to give a completely general definition of success in objective biological terms. In the case of hunger, success consists in the eating of food. Ordinarily the successful act results in a cessation of the persisting stimulus S_1. In the case of hunger, S_1 is generally considered to be the cramping of the walls of the upper digestive tract.

8. The trials cease after success has been attained simply because success terminates the stimulus (S_1) which evokes the trials.

9. Erroneous acts continue to be made even after the correct solution has been 'discovered' one or more times by successful trials because the reinforcement, by success, of a weak excitatory tendency is not always great enough to make it equal in strength to excitatory tendencies which were originally more potent and which have had time to recover greatly from the effects of experimental extinction suffered just previous to the successful reaction.

10. The erroneous reactions become less and less frequent as the trial-and-error process continues, because the basic superiority in the strength of the excitatory tendencies leading

to erroneous responses becomes less and less dominant. This in turn is owing (*a*) to the action of experimental extinction which continually weakens such erroneous reactions as chance to become functionally dominant, and (*b*) to the action of reinforcement which strengthens the excitatory tendency which, when dominant, evokes successful responses. Ultimately this process must lead to a state in which the successful excitatory tendency will be dominant at the very outset of a behavior cycle.

If this first case of success on the initial trial of a behavior cycle should chance to take place under circumstances such that the spontaneous recovery from extinction by the unsuccessful tendencies had not had time to take place (as might have happened if Cycle II. had begun very soon after the conclusion of Cycle I.), then we should expect to find errors made repeatedly after one or several perfect initial performances had occurred.

11. One problem is more readily solved than another for a given organism because in its particular stock of reaction tendencies the one tending to the successful reaction chances to be relatively stronger than in the other problem situation. In case the excitatory tendency evoking the successful reaction chances to be dominant at the outset, the correct reaction will be made at the first trial and no errors whatever will occur. On the other hand, the same problem may be more difficult for one organism than another, both of which can ultimately master it, because the previous history of the two organisms has so conditioned them that the successful tendency is relatively more dominant in one than in the other. Such relative similarity in the difficulty of problems for different specimens of a given type of organism as actually exists presumably depends upon the relative similarity in the stimulating situations encountered in their lives. This is usually considerable.

12. From the foregoing, it is obvious that trial-and-error learning, while 'blind' in the sense that it is not assumed that there is available for its guidance and control any disembodied soul or spirit, is *not* blind in the sense that it does not operate according to recognized principles. In the first place, the

trials are not made from the total repertory of the organism, but from only those movements which have by previous stimulation become conditioned to one or another stimulus component of the problem situation. This fact at once automatically limits enormously the number of trial reactions from which selection must be made, and thus largely accounts for such efficiency as it displays. In the second place, of those acts which may be evoked by the stimulus situation, it seems reasonable to expect that in the long run the stronger excitatory tendencies will be more likely to evoke successful reactions than the weaker, and the weaker ones than those reactions within the repertoire of the organism, which have not become conditioned at all to any component of the problem stimulus complex.[5] Since the trial acts are evoked in the order of their strength, this factor will also greatly favor an early success over a mere random sampling from the possible reactions of the organism. It is true that such a system would not always succeed early, and might fail completely of the solution of a problem. Unfortunately this also agrees with the facts of life. Problems are often solved only after much delay, and not infrequently they are not solved at all.

V

From the point of view of the longevity of hypotheses, it is extremely dangerous for them to become thoroughly definite and specific. The very definiteness of an hypothesis makes it possible to determine with relative ease whether its implications agree with the known phenomena which it proposes to explain. In case of failure to conform, the unambiguous nature of the comparison is peculiarly fatal. Worse yet, an unambiguous hypothesis is likely to permit the deductive forecast of what should be observed under various experimental conditions which may as yet be untried. A single well-planned experiment may at any moment yield results quite different from the deductive forecast, and thus topple

[5] Space is inadequate to elaborate this point. It is touched on briefly, however, in 'A functional interpretation of the conditioned reflex,' this JOURNAL, 1929, 36, p. 498.

the entire hypothetical structure. This, of course, is all quite as it should be. The healthy development of a science demands that the implications of its hypotheses be deduced as promptly and unambiguously as possible. This will make it possible for them, if verified by experiment, to be incorporated into the structure or system of the science; or, if found to disagree with experimental findings, the hypotheses may be recast or simply discarded as errors in the long trial-and-error process of system construction. At the least, such hypotheses may be credited with the virtue of having stimulated experimental research. But if an hypothesis be so vague and indefinite, or so lacking in relevancy to the phenomena which it seeks to explain that the results neither of previous experiments nor those of experiments subsequently to be performed may be deduced from it, it will be difficult indeed to prove it false. And if, in addition, the hypothesis should appeal in some subtle fashion to the predilections of a culture in which it gains currency, it should enjoy a long and honored existence. Unfortunately, because of its very sterility and barrenness in the above deductive sense, such an hypothesis should have no status whatever in science. It savors more of metaphysics, religion, or theology.

Substantially the only significant criticism of the stimulus-response, or mechanistic movement in psychology, has been made by members of the Wertheimer branch of the *Gestalt* school, notably by W. Köhler and K. Koffka, particularly the latter. This painstaking criticism of theoretical stimulus-response constructs has been a distinct service to science. Better still, they have put forward a quite different set of principles to explain the same phenomena, which are proposed as alternative, because assumed to be superior, concepts. The issue has thus been joined in a manner quite frank and deliberate. Best of all the contest, instead of taking place in a field of pure speculation where a decision can rarely be reached, is to be conducted in the laboratory where the decision must ultimately be submitted to the impartial arbitration of the facts. That hypothesis, or set of hypotheses, which can show the highest achievement in the things which are recognized by

scientists as the functions or virtues of hypotheses must in the end be judged the superior.

As a beginning in this direction there may be considered the above theoretical construct concerning one extremely limited type of trial-and-error learning. Here the question at once arises: Are the concepts or principles, by which the *Gestalt* psychologists would explain the kind of behavior under consideration, of such a nature that answers to the above questions can also be deduced from them? The present writer is frank to confess that such concepts as closure, *Pragnanz*, *Einsicht* and the like appear to him either too vague or too general to permit any significant deductions whatever to be drawn. He is quite free to admit, however, that this may be due merely to his failure to grasp the true significance and virtue of these concepts. The real test is whether the *Gestalt* psychologists themselves can do so. It is entirely possible, of course, that they may repudiate in whole or in part the very existence of conditions implied in the questions propounded. In that event it would seem fair to expect an exhibition of the deductive explanation of parallel phenomena as they conceive them to exist.

VI

It is admitted on all hands that one of the very best tests of an hypothesis or explanatory system is to deduce correctly the result of experimental observations not yet made, particularly when the latter are made by observers disinterested in the outcome. This is a severe test, but it is a fair one, and no system should shrink from it. A number of such possibilities lie sufficiently near to the range of the very simple conditions assumed above for first-approximation forecasts of the outcome of certain experimental procedures to be ventured. Space is lacking for the presentation of but one of these. We shall make our own deduction from the same set of principles already employed. A friendly invitation is extended to the *Gestalt* psychologists, and to such other schools as put forward distinctive theories of learning, to exhibit in similar detail a similar deduction from their own principles. If no such

deductive forecast can be derived, there will be an indication of immaturity, possibly of inadequacy. If a deduction is evolved in which recognized stimulus-response principles are employed, the indication will be that the psychology in question is not so distinct as might otherwise have been supposed. If a rigorous deduction from genuinely distinct principles should appear, but one in which the same outcome is arrived at as by the stimulus-response principles here employed, an extremely interesting situation of parallelism would be presented, which might very well be mutually illuminating to all parties to the discussion. Finally, if any two deductions should arrive at quite distinct forecasts as to the outcome of the experimental procedures, the laboratory may be evoked as the final court of appeal. Indeed, the laboratory must pass the final verdict even if there were no difference of opinion whatever.

Forecast: In cases of relatively simple trial-and-error learning by mammalian organisms below the anthropoids where, as above, but a single act is required for success; where the several trial acts are relatively distinct and uniform; and where the first one-fourth of the behavior cycles required for complete learning have been both fairly protracted and in fairly close succession: it is predicted that there will be a tendency for the proportion of erroneous acts (R_z and R_y) to successful acts (R_x) to be greater at the first trial act of new cycles when the new cycle begins a relatively long time after the conclusion of the preceding one than when it begins relatively soon after.

Deduction: From Table I it is quite obvious that if Behavior cycle II. should begin at once after the conclusion of Cycle I., R_z will be dominant since the status of the excitatory tendencies will be:

$$x = .9$$
$$y = .5$$
$$z = 1.3$$

But if, instead, an hour intervenes, the relative strength of the tendencies will be:

$$x = 1.2$$
$$y = .8$$
$$z = 1.3$$

At this point R_z and R_z are about equally likely to take place. Or if, as a third alternative, still more time elapses between the close of Cycle I. and the beginning of the next cycle, R_x and R_y will both become progressively more dominant over R_z until at length we shall have the condition obtaining at the first trial act of Cycle II. as shown in Table I.

It must be especially emphasized that the type of learning here considered is not only very simple, but very special in its simplicity. Its nature is perhaps best indicated in the early parts of sections II. and III. It naturally will require some ingenuity fully to satisfy these conditions in an experiment. In particular it may be difficult to set up an experimental situation where all the components of the stimulus complex, except a single dynamic core (S_1), will remain practically neutral throughout the reaction sequence.[6] However, ingenious experimentalists will be able to approach those conditions closely enough to make possible significant comparisons of results.

VII

In conclusion it may be observed that the behavior deduced above, particularly the persistence of effort at solution by means of varied response, is one of the most commonly remarked differences between behavior, usually called psychic or mental, and that of ordinary automatic machines. Indeed it is common, by way of contrast, to call such behavior 'intelligent' and 'purposive.' It is the belief of the present author that these latter terms represent extremely important aspects of mammalian behavior, but that instead of being

[6] In this connection it may be asked why the implications of such a simplified situation should be studied. The answer is found in the history of mathematical procedures. It is often possible to understand the implications of simple situations when, at the outset, the understanding of more complex situations would be impossible. The previous solution of simple situations should make the later solution of progressively more complex situations possible. There is reason to hope that this shall prove to be the case in the present instance.

ultimate entities, all may be derived from certain combinations of more basic principles. It is believed, for example, that the account sketched above in section III. deduces a type of behavior which, if observed in an animal, would be called purposive by most psychologists though it does not show the type of purpose involving plan.[7]

Moreover, if the type of explanation put forward above be really a sound deduction, it should be a matter of no great difficulty to construct parallel inanimate mechanisms, even from inorganic materials, which will genuinely manifest the qualities of intelligence, insight, and purpose, and which, in so far, will be truly psychic. Such a mechanism would represent a radically new order of automaticity, one not yet dreamed of by the ordinary designer of automatic machinery. That such mechanisms have not been constructed before is doubtless due to the paralyzing influence of metaphysical idealism. The appearance of such 'psychic' mechanisms in the not very remote future may be anticipated with considerable confidence. Dr. H. D. Baernstein, in collaboration with the present author, has already succeeded in constructing an electro-chemical mechanism which shows the more important of the phenomena of the simple conditioned reflex.[8] There has also been constructed a model which manifests the phenomenon of simple rote learning. It is not inconceivable that 'psychic' machines may ultimately play an appreciable rôle in the life of industrialized communities. On the side of psychology it is possible that these mechanisms may dissolve the age-old problem of the opposition of mind to matter by practically demonstrating the characteristic mechanisms by means of which matter manifests the forms of behavior called psychic.

[MS. received November 13, 1929]

[7] It is a plausible hypothesis that the type of purpose involving plan and fore-sight (or fore-knowledge) requires, in addition to a persisting dynamic core in the stimulus complex, as in the case elaborated above, a flexible symbolic habit system. The implications of such an accessory habit system for trial-and-error learning, as well as further implications of the persisting-stimulus principle, are reserved for later examination.

[8] C. L. Hull and H. D. Baernstein, A mechanical parallel to the conditioned reflex, *Science*, 1929, 70, pp. 14–15.

PHYSICAL RELATIVITY AND PSYCHICAL RELATIVITY

BY OLIVER L. REISER

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Up to the present it has been assumed that Einstein's theory of relativity is applicable only to physical phenomena. Relativity principles have not been admitted to have any significance for biological or psychological phenomena. To be sure, W. Köhler and the other gestalters have had much to say about the *relativity* of 'sensations' and conscious experiences generally. These investigators have also speculated concerning the relative properties of the physiological systems which presumably underlie phenomenal patterns. But this relativity of physico-chemical properties has not been regarded as in any sense an expression of Einsteinian relativity.

The view of the gestalters, however, presents but one interpretation. Another possible conception is found in the attempt to assimilate psychological relativity to more general physical principles. We then ascribe the inability of the gestalters to arrive at a synthesis of physical and psychological phenomena to the particular type of biochemical theory which they have embraced. If we search for other physiological analogies which might conceivably provide the material substratum for conscious processes, it appears that one might exhibit psychological relativity as the subjective counterpart of Einsteinian relativity of brain processes. To illustrate and support this view let us compare the type of explanation employed by Professor Köhler with another type of explanation which promises such a synthesis as we have suggested.

Consider the phenomenon of simultaneous color contrast. Here the chromatic properties of the perceived pattern are not 'absolute,' but are 'relative' to the total situation. For

instance, a gray square set within a green background no longer appears as gray, as it did before it was thus situated. Again, when a green and red surface are set side by side the color of each along the boundary lines appears to be intensified. Now according to Köhler, in optical processes the perceived contours are preserved by 'forces of contact' which depend upon the differences in the properties of the sides of the contours in the brain. Köhler is therefore forced to seek for the physiological condition of the relative character of the perceived color qualities in the relative differences in the properties of the chemical reactions in the underlying brain fields.

He points out in his recent book, 'Gestalt Psychology' (p. 218), that in dealing with solutions with different ionic concentrations the answer to the question of whether a given solution is 'electropositive' or 'electronegative' depends on the relation of this given solution to the other solutions with which it is being compared. Here we find that the 'gestalt' property of an electro-chemical whole has the same features as the brighter and darker side of a sensory pair of gray stimuli.

Köhler does not identify the relativity of color qualities with the relativity of the ionic concentrations of solutions. He does, however, point out that differences in chroma are usually associated with differences in brightness, which act as effective forces of separation. So far as I am aware, this is as close as Köhler has come to stating the physiological condition of color perception.

Now let us consider the other type of explanation of this kind of phenomenon, which rests upon a different sort of physical analogy and which comes closer to a true physical relativity.

The phenomenon of the Doppler effect is well known to physicists. It appears in sound as well as in light transmission. We are all of us familiar with this phenomenon in the experience of hearing the pitch of a bell at a railroad crossing change as the train we are in approaches, overtakes, and passes the ringing bell. When the train is approaching the crossing the source of sound is moving (relatively) towards us. Accordingly, we overtake a greater number of sound waves per

unit interval of time, and hence the pitch appears to be higher; as we recede from the bell fewer waves strike our ear per unit interval of time and the pitch of the sound appears to be lower. The quality of the sound depends upon the relative motion of the perceiver with respect to the source of stimulation. In other words, a variation in the relative velocities of the observer and the thing observed produces the same effect as an objective variation of the stimulus when the observer is at rest with respect to the source of the stimulus. Now, we ask, is there anything in the brain which could correspond to the relative acceleration and deceleration in the foregoing case?

In surveying the possibilities we come across the interesting theory of nerve conduction which supposes that each nerve cell has its characteristic frequency, and that nerve conduction is the process whereby a stimulus is passed from one cell to another with which it is in 'tune' (or *isochronous*). Now let us see how this theory of L. Lapicque might be related to physical relativity.

At the outset we must note that the way in which a present stimulus is perceived is a function of the previous history of the organism. Furthermore, the manner in which a thing is perceived is related to the other activities going on in the brain at the same time. In terms of the chronaxic theory of nerve conduction this may mean that the activities of a given set of neurones are related to the activities of other neurones which are also active. Dr. H. M. Johnson has suggested that something analogous to the acoustic phenomenon of 'forced vibration' may be operative here. Under such conditions the same objective stimulus will not arouse the same response; its qualitative variables will not be reproduced in consciousness in the same way.

The brain would interpret the old physical stimulus as a new conscious quality. Whether, in the case of vision or audition, this new quality would appear to be of a higher or lower pitch (frequency) would depend on whether the rhythm of discharge of the cell had been raised or lowered relative to its own former frequency and to the frequencies of the adja-

cent nerve elements. Perhaps this type of relativity is illustrated in the realm of taste experiences, where, as is known to all, the eating of an apple after you have eaten candy makes the 'sweetness' of that candy appear quite different from what it would have been if eaten before the candy.

Because of the lack of knowledge of the mechanisms of brain processes it is difficult to work out the details of this conception. But a possible application and test of the view might be found in the alleged production of visible radiation from excited nerve fiber. From the fact that when you view a band of red light in a dark room you see the band surrounded at the ends with reddish blue arcs, Dr. Christine Ladd-Franklin infers that we are actually seeing the light emitted by the nerve elements active in responding to the red band. But Dr. Ladd-Franklin has not been able to offer any explanation of the color of this experience, which involves higher frequencies of light than the original stimulus. If this is a case of fluorescence, as Dr. Ladd-Franklin suggests, it violates Stokes' law, which states that the emitted light is of a lower frequency than the absorbed light which produces the radiation. If this law has no exceptions, the red band ought to be surrounded by infra-red light, which, however, would be invisible.

But if we introduce relativity considerations it might appear that the light emitted by active nerve fiber would appear as of one color when seen subjectively, though when studied by external observation it might be of a quite different quality—in the visible region, infra-red, or even ultra-violet.

The working out of such a scheme is a complicated problem. But even greater difficulties arise when we consider the fact that in physics shifts in the spectrum lines occur under other conditions. In astronomical observations displacements of the Fraunhofer lines towards the red or the violet end occur under the following conditions: (*a*) When a star is receding or approaching the observer the shift is, respectively, towards the longer or shorter wave phenomenon. This is the Doppler effect in light perception. But (*b*) there will also be a displacement of the lines towards the red end of the

spectrum when an atom (in the sun, for example) is in a gravitational field, which slows down the atomic vibrations. This phenomenon constitutes one of the tests of the theory of relativity.

We have already suggested the corresponding phenomenon in the brain which might be analogous to the first type of physical phenomenon. If we could discover any biological correlate of the second type of relativity effect this would make the analogy between physical and psychological relativity much closer. Curiously enough, there is some theoretical justification for believing that this second phenomenon might also be involved in brain processes. Let us see.

As Professor A. S. Eddington has pointed out in his book, 'The Nature of the Physical World' (p. 22), the same body can both give and not give a magnetic field. An electrically charged body at rest on the earth possesses an electric field but no magnetic field. A moving charge constitutes an electric current which, in accordance with the laws of electromagnetism, gives rise to a magnetic field. There is no magnetic field for a charged body at rest on the earth, but since the earth is in motion a nebula-dwelling physicist would detect a magnetic field even though the instruments on earth would not show a magnetic field.

In order that such considerations could be at all relevant to brain processes it would be necessary to demonstrate the existence of fields of force associated with cerebral processes. It is well known that differences of electrical potential exist. It is also inferred by some, P. Lazareff for example, that since we have electromotive forces present in the brain, electromagnetic fields must also accompany brain processes. Some would be reluctant to ascribe much importance to such fields of force, since they cannot be very strong, but we must not forget that such fields might be strong enough to initiate processes which then proceeded on their own free (potential) energy. And here is where relativity enters. Köhler has suggested that the attraction of two similar mental processes for each other is analogous to the attraction of two parallel wires having electrical currents traversing them. According

to the present view, if two brain processes occurred of sufficient rapidity with respect to intermediate processes in the brain, a relative field of force might be set up in each such process, and this might underlie the 'attraction' of two similar mental processes for each other.

Gestalt psychologists have pointed out that parts of the brain which are structurally disparate may be dynamically unified into functional wholes. Since these investigators propose to substitute dynamic explanations for structural explanations, may we not just as well ground the relativity of conscious processes in the physical relativity of electromagnetic fields of force?

There is one important respect in which Köhler's view appears to be superior to the foregoing conception. In his speculations Professor Köhler makes use of thermodynamic principles, and this has the merit that it permits him to explain the characteristic fact of consciousness, the unidirectionality of experienced time, in terms of the irreversibility guaranteed by the second law of thermodynamics. So far as I am aware, the gestalt psychologists have never committed themselves to this explanation of the one-way feature of conscious time. But in defense of the present view it might be pointed out that the foregoing relativistic interpretation might be extended to include an explanation of this apparently absolute fact. This, however, must be considered on some other occasion. At the present time only one other aspect of the foregoing ideas can be considered.

The question has long been discussed as to whether, when we both look at a common green object, the green you see is exactly the same as the green I see. The inability to decide this question is designated by the term, the privacy of consciousness. This subjectivity of sensations is one of the foundation stones of the behavioristic system, which holds that science has no concern with facts which are not objective or socially accessible.

By way of reply to the behaviorists some daring introspectionists have argued that some day it may be possible to establish, through surgical means, an anatomical con-

tinuity between brains in such a way that an individual could be conscious of another's conscious response as well as his own, thus enjoying the privilege of making the comparison which the behaviorist insists is necessary to an objective science. Aside from the practical difficulties which render such an experiment well nigh impossible, there is, from the foregoing point of view, another difficulty which would have to be surmounted.

If it is true that the way in which a person sees anything depends upon previous history as well as upon the present activities of adjacent areas, then to get another person's conscious perceptions of color one would have to use the other person's entire cerebral, or even entire bodily, apparatus. The reduplication in your own brain of the rhythm of discharge of another's cortical analyzer introduces the proper frequency of your own nerve cells; hence you can never be sure that you are seeing the color green exactly as some one else sees it. In this respect consciousness will always be the private possession of each individual.

Summary

Psychological relativity and physical relativity, which are established doctrines within their own domains, have hitherto not appeared to possess any significance for each other. But since the brain is itself a physical system in which relative movements occur, the suggestion arises that psychical relativity might be assimilated to more general physical principles. Employing certain analogies from physical systems, this hypothesis is made more definite and plausible.

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THE GENETIC INTERRELATION OF INSTINCTIVE BEHAVIOR AND REFLEXES [1]

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In terms of introspective psychology it appears that instinct connotes an inherited or innate faculty or agency which determines certain behavior patterns that are known as instinctive. In behavioristic psychology, on the other hand, the concept of an innate faculty or agency is abandoned and the use of 'instinct' in the generic sense is supplanted by its use in the particular sense, that is to say, the behaviorist speaks of this, that, or the other instinct, which is in each instance an objective behavior pattern as such.

As regards their essential nature and origin the behaviorist regards instincts as integrations or syntheses of reflexes, while it seems that reflexes are regarded by introspectionists and behaviorists alike as purely physico-mechanical components of behavior of the simplest order. It is the purpose of this paper to present these two concepts of instinct as related to reflexes in the light of the development of the behavior of the individual.

In so far as the development of behavior is known in vertebrates, all reflexes emerge as partial or local patterns within an expanding or growing total pattern that normally is from the beginning perfectly integrated.[2,3] They become partial or local only overtly. They are inherently components of the total pattern or they are under its dominance.

[1] Read before the Ninth International Congress of Psychology, New Haven, Conn., September, 1929.

[2] G. E. Coghill, Anatomy and the problem of behavior, Cambridge University Press, 1929.

[3] G. E. Coghill, The early development of behavior in amblystoma and in man, *Arch. Neur. & Psychiat.*, 1929, 21, pp. 989–1009.

An instinctive reaction, on the other hand, is the total pattern overtly in action. For example, the start of the new-born infant in response to a loud noise is regarded as an expression of the instinct of fear, whereas its reaction to a touch on the eyelid is regarded as a reflex. But in the fetus of about ten weeks the reaction to a touch on the eyelid involves movements of the legs.[4] This is a very extensive reaction, involving as it does the medulla oblongata and spinal cord throughout practically their entire length. It is essentially the same reaction as the start of the young infant to a sudden loud noise. Yet the infant's reaction is called instinctive while that of the fetus is regarded as reflexive. Both responses are, however, essentially total reactions. But by the time of birth, reaction to touch on the eyelid has become individuated out of the total pattern and has become a local reaction, while it is only considerably later that the reaction to a noise becomes individuated into a partial pattern so that the child may, for example, turn the head toward the source of sound and in other respects remain overtly quiescent.

The process of individuation of the partial pattern out of the total pattern is the same in these two instances: it consists in progressive restriction of the zone of stimulation adequate for the response, and in progressive reduction of the extent of muscular reaction. The end results, also, are the same in both cases, for both contribute to the adaptation of the organism as a whole to its environment. Obviously, therefore, since the reflex and the instinct develop according to the same law and serve the same purpose they can not be regarded as two distinct categories of behavior. Their apparent difference with reference to the magnitude of the pattern is simply a function of growth.

The nervous mechanism of the total reaction of the individual as a whole is a perfectly definite thing in those lower vertebrates in which the development of the nervous system as correlated with the development of behavior is best known. This mechanism is the first to become functional

[4] M. Minkowski, Neurobiologischen Studien am menschlichen Foetus, *Handb. der biol. Arb.*, 1928, Abt. 5, Teil 5 B, Heft 5, p. 511.

in the nervous system, and, in early life, it controls all behavior. As it grows through the organism the behavior pattern expands accordingly, and, after various secondary mechanisms have become established in the nervous matrix, this primary mechanism persists as a clearly recognizable component of the nervous system, and still sustains its typical relation to the effectors. Meanwhile there grows into it a profusion of terminals from all sources of sensory excitation. Thus a stimulus from any sense organ may excite the total reaction.[5]

How the mechanism of the total reaction is the instrument by or through which the organism as a whole maintains the integrity of its behavior pattern during development has been demonstrated.[6] It is by the growth of neurones after they have begun to function as conductors. The mechanism of the total behavior pattern is, therefore, the agency of integration which we seek in the interpretation of reflexes and instincts. This agency acts by means of its growth; and it grows according to its own intrinsic pattern. Within the limitation of this intrinsic pattern of growth it is autonomous both in its reaction to its environment and in its action upon its environment; and in this autonomy is the natural source of initiative or freedom in behavior.

There is, then, in a certain sense, a biomechanistic equivalent of instinct as a faculty or agency. It is an intrinsic thing as regards the organism; and not being vitalistic in the ordinary sense it is susceptible to scientific inquiry. In it as a dynamic system may be found, I believe, a natural basis for the interpretation of reflexes and instincts, and for that individual initiative, autonomy or freedom which appears to be essential to psychology and sociology as sciences.

[MS. received November 19, 1929]

[5] G. E. Coghill, Correlated anatomical and physiological studies of the growth of the nervous system of amphibia. V. The growth of the pattern of the motor mechanism of amblystoma punctatum, *J. Comp. Neur.*, 1926, 40, no. 1, p. 47.

[6] G. E. Coghill, Anatomy and the problem of behavior, Cambridge University Press, 1929.

'PRACTICAL LOGIC' AND LADD-FRANKLIN'S BLACK

It is well known that up to the present Ladd-Franklin's theory of color vision appeals more to the psychologist than to the physicist. As a matter of fact, it stands or falls with sensory data; it cannot afford to violate the elementary facts of introspection even for the sake of 'practical logic' any more than it can disregard the Helmholtz-Koenig facts of color mixture.

The writer happens to be one who thinks with Professor Warren that Ladd-Franklin has proposed "the most satisfactory explanation so far suggested," yet its chief difficulty "is to account for the sensation of black."[1] Ladd-Franklin, however, would insist that "to put the blackness-sensation into the same category with those visual sensations which are the response to the action of light on the retina" is not what "the nature of the phenomena calls for." "The sensation of blackness is the psychic correlate of a very different physical situation, namely that of no stimulus whatever—a situation which does not vary in intensity. But the crucial difficulty that arises when one tries to put the blackness-sensation into the same category with the other visual sensations is that a black-white, a black-blue, etc. (unlike a green-blue, a red-blue, etc.), cannot be made to change its intensity without at the same time changing its quality. . . . The simplest way in which to understand this is to suppose that the 'black' physiological situation, exactly like the 'black' physical situation, is something that cannot change in intensity."[2]

These statements seem to the writer to be unnecessarily complicated and leave one confused as to where fact ends and hypothesis begins. Now one of the points of contention between the Helmholtz and the Hering types of theory is exactly whether blackness is the psychic correlate of no stimulus whatever. It is a point on which quite a few psychologists have taken too much for granted. Indeed

[1] H. C. Warren, Human Psychology, p. 181.

[2] C. Ladd-Franklin, The uniqueness of the blackness-sensation, *Amer. J. Physiol. Optics*, 1925, **6**, p. 453. I am quoting from 'Colour and colour theories,' p. 211.

Titchener once had to call Ward's attention to it. "The 'cessation or inhibition of the intrinsic light' is an hypothesis of McDougall's; and it is an hypothesis to be considered; but it has hardly yet attained the rank of established fact."[3] So Ladd-Franklin does not stand alone in this precarious position except for the fact that one of her followers has recently formulated her ideas in such a threadbare fashion that nonsequacious logic shines through it. This, for instance, was supposed to be "the simple fact in the case": (1) Blackness is due to zero stimulation. (2) There is no possibility of varying the amount of black.[4] Whether the second statement can stand by itself we shall see. In so far as these postulates essentially represent Ladd-Franklin's position and apparently were endorsed by herself, we might mention that we fail to see any psychophysical necessity or cogency which holds them together.

Returning now to the 'practical logic' of her own statements quoted above, it seems her theory must first of all account for the very existence of the 'black-white,' 'black-blue,' etc., which are indeed plain simple facts for the psychology of color. Black, let us take for granted, is the one-intensity sensory correlate of no stimulus. Should we then imagine that 'black-blue' is a dual correlate of no stimulus and of stimulus for blue? Speaking arithmetically, any number is no doubt equal to that number plus zero but here we are made to believe that zero adds to the magnitude of that number!

Could it be possible that Ladd-Franklin is simply using the term 'black' in the broad sense as Hering used it? Then it cannot be the constant background sensation whose uniqueness gives her trouble. Ladd-Franklin must have realized that a black-blue cannot be obtained by decreasing the intensity of a spectral blue. It must be 'blackened,' as Parsons puts it,[5] like the browns and olive-greens. Blackening it by degrees also, one will have to describe the series in the plural; there is not just one black-blue. This varying phenomenon which one easily observes in simultaneous contrast and the negative after-images appears so unequivocal that it leaves no room for any one-intensity theory.

However, with common sense restored to the blacks, one begins to doubt the unqualified truth of the statement, "A black-white, a black-blue, etc. . . . cannot be made to change its intensity with-

[3] E. B. Titchener, A note on the sensory character of black, *J. Phil., Psychol. and Sci. Meth.*, 1916, 13, p. 119.

[4] G. M. Michaels, Black, a non-light sensation, PSYCHOL. REV., 1925, 32, p. 249.

[5] J. H. Parsons, Introduction to colour vision, p. 25.

out at the same time changing its quality." By 'quality' is meant either 'chromaticity' which is conventionally known as saturation or the assumed fact that black is black which cannot admit of becoming brighter. If we agree to beg the question, there is nothing to be said. On the other hand, if the point to be settled is whether within certain limits a black-blue can become brighter and still look like a blackish blue, the writer for one must insist that it can, without violating common sense or strict systematic observation. The same may be said of the black-red-yellows, the black-blue-greens, the black-whites, etc.

But after all, is this 'crucial difficulty' of Ladd-Franklin's really consequential in systematically locating blackness? The author seems to be rather partial with whiteness. Is it not also to some extent true that a white-black, a white-blue, etc., cannot be made to change its intensity without at the same time changing its quality? If "the only way you can make a blue-black brighter is by making it at the same time *bluer*," so also a blue-white cannot be made darker except by making it at the same time *bluer*. As examples the blue-whites and blue-blacks are rather unsatisfactory, there being more of the former, so that blackness may on that account look 'unique,' but between this difference in discriminability and the assumed one-intensity level there lies a long stretch of hypothesis.

Needless to say, the so-called cortical theories of blackness seem to the writer to be further removed from the facts and too much fabricated. Black we see when there are no 'cosmic holes' to fill out. Complete darkness-adaptation comes as near to a cosmic hole as we can make it without exoculation, but then it is not black we see.

Thus blackness is unique not so much as a sensory datum but rather on account of its extraordinary position in theory-making. It is not exaggerating to say that the Hering theory, in spite of its physiological vagaries, still recommends itself to scientific thinking only because it alone among the more prominent rivals does more than lip service to the blacks. Background sensation, non-light sensation, even the negation of all sensation—for theory-construction at least—all can serve just one function, to fill out the 'cosmic holes' of logic. There is really no difference between Ward and Ladd-Franklin except perhaps an 'existential' one. These seemingly insignificant blacks now occupy such an inconvenient position between the warring camps that it pays to be sometimes reminded of them.

In a recent theory proposed by Forbes,[6] for instance, a structure which in its groundwork is very much like Hering's, the author first condemns Young and Helmholtz for failing to interpret 'violet and *brown*.'[7] The black sensation is, however, assigned to 'post-retinal structures'[8] which are left unspecified. Only it is "assumed to be the result of a central process—inhibition by the activity of neighboring neurones or less probably to a reflex cutting off of the restoring substance."[9] How neural inhibition or the cutting off of a chemical substance can occasion the 'positive sensation of black' we still wish to know. It taxes the imagination more than did Hering's anabolic process in so far as the author himself still wavers between the retina and some possible central process as the place of origin. One rather expects he would find difficulty with the rebellious browns.[10]

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[6] W. T. M. Forbes, 'An interference theory of color vision,' *Amer. J. Psychol.*, 1928, 40, pp. 1–25.

[7] Idem, *loc. cit.*, p. 1. Italics mine.

[8] Idem, *loc. cit.*, p. 3.

[9] Idem, *loc. cit.*, p. 5.

[10] Idem, *loc. cit.*, p. 13.

A CHART TO ILLUSTRATE THE FUNDAMENTAL DISCREPANCIES OF COLOR STUDY IN PHYSICS, ART AND PSYCHOLOGY

The study of color is generally confusing. The discrepancies found in physics, art and psychology offer no end of complication to the student who is conscientious enough to strive for a clear and exact understanding. Explanations and descriptions are difficult to reconcile. Color charts appear to offer no solution and to hold not the slightest suggestion that the subject of color has any appreciable coherence in its three aspects.

As a rule the first enlightenment in color study is gained from the paint box, where red, yellow and blue are selected as primaries and made to form all other hues in various mixtures. The artist's triad is set up as the first concise graph of color relationships. In a second step, an education in physics presents a new set of facts in which the triad idea is also used, but where the primaries selected are red, green and blue. Here likewise—but in the medium of light—all hues are formed. Then, in a third step, the psychologist adds his tetrad of red, yellow, green and blue and accomplishes his color mixtures on the color wheel. With these facts and these three sets of primaries before him, the student finds himself in the midst of a rather complicated state of affairs.

It is, of course, necessary to clear up this confusion. Too often the student is likely to conclude that the real truth about color rests with one or another of three contentions, and that choice is optional. The usual result is that one of three aspects alone is kept in mind and remembered as the essential law of color.

Yet the facts about color in pigments do not conflict with the facts about color in physics; nor does psychology, with its four visual primaries, dispute any of the claims made by the artist or the physicist. There are, in fact, three separate aspects of color study, and each is unique in itself. Any device that will facilitate a better knowledge of these differences naturally will have value in clarifying the study of color in psychology and isolating it from the conclusions of the artist and physicist—without inferring that the one viewpoint is right and the others wrong.

Thus the charts illustrated, while entirely general in scope, provide an efficient graph of those color relationships encountered.

The major facts about color may be conveniently illustrated and described with them.

In Fig. 1 is the color circle consisting of red, yellow, green and blue, and the four common color-blends, orange, yellow-green, blue-green and purple.

In Fig. 2 the triad of the physicist is imposed on the color circle, with its three points resting on the physicist's primaries, red, green and blue. In this figure the triad properly shows yellow to be a color-blend, formed by mixtures of red and green. Then, in the center of the triad, a complementation point (*A*) is established. From this point opposite hues are determined—blue-green being the complement of red, purple the complement of green, and yellow the complement of blue.

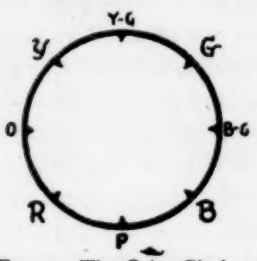

FIG. 1. The Color Circle.

FIG. 2. The Physicist's Triad.

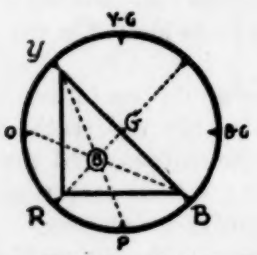

FIG. 3. The Artist's Triad.

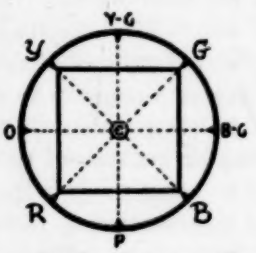

FIG. 4. The Psychologist's Tetrad.

FIG. 5. The Completed Chart.

In Fig. 3 the triad of the artist is imposed on the color circle. Its points rest on the three pigment primaries, red, yellow and blue. Here green becomes a color-blend formed by mixtures of yellow and blue. Complementation point (*B*) reveals orange to be the complement of blue, purple the complement of yellow, and green the complement of red.

In Fig. 4 the tetrad of the psychologist is imposed on the color circle. Here there are four primaries touching red, yellow, green and blue. Complementation point (*C*) gives the opposites as red and green, yellow and blue, orange and blue-green, and yellow-green and purple.

In Fig. 5 the triads of the physicist and the artist, and the tetrad of the psychologist are all imposed at once on the color circle. With the complementation points retained, the three aspects of color study are made to show an agreeable coherence. Differences as to primaries, color-opposites and color-blends are exhibited simultaneously, and many of the bewildering inconsistencies of color are clearly contrasted. Explanations of color facts, through the use of charts such as these, should aid materially in removing major confusion and in making the psychologist's viewpoint, as it differs from the viewpoints of the physicist and the artist, entirely clear.

The main laxity of the chart—which must be forgiven in view of its encompassing utility—is found in the selection of red and green as complements in the tetrad of Fig. 4. This agrees with Hering, but disagrees with Ladd-Franklin. However, no chart can hope to surround all color facts, no less settle the controversies of psychologists. Thus the detail will have to be overlooked and the chart accepted for its larger purpose.

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A NEW VISUAL PHENOMENON—THE CIGARETTE ILLUSION

If the glowing end of a cigarette or cigar is fixated in daylight, in a perfectly dark room, or under ordinary indoor illumination it will appear orange in color with the red component usually predominating. But if we view the glow under a good red incandescent lamp, we will be surprised to find that the red component has entirely disappeared and the burning end looks green or yellowish-green! Under green illumination it appears even more reddish than

in daylight, while under yellow and blue illumination the glow is orange rather than anything else. It thus appears that our strange result occurs only under *red* illumination. The effect is best obtained if the red lamp is screened from the eyes by the hand or a piece of cardboard, and the observer's eyes are at a distance of from twenty to twenty-five inches from the cigarette. We may summarize the factors making for or against the illusion by pointing out three conditions affecting it: (1) the position of the source of light and the cigarette with reference to each other and to the observer; (2) the intensity and goodness of the red light used as the illuminating source; (3) individual differences: some individuals see more green in the glow than others, but practically all observers agree that the red has disappeared and given place to green, yellowish-green or greenish-yellow.

In writing several colleagues about this phenomenon I have received a number of explanations which would occur to the psychologist. A consideration of the factors which do not influence the phenomenon and some experiments designed to test the various hypotheses show, I believe, that the ordinary explanations that may be advanced to account for our observations fail to hold.

In the first place we must note that by spectroscopic analysis more red and yellow are actually known to be emitted from the burning cigarette than green and blue. We should therefore expect that if we throw red on red the result would be a red of even better hue. This is actually found to be the case if we use color discs of red and green and rotate them upon a color mixer and illuminate them with red light,—the result is red. Moreover, if we use a yellow illuminant and rotate blue and yellow upon our color mixer, we shall see a good yellow. The complementary color does not come out as in the case with our glowing cigarette. Burning punk does not give the phenomenon, while a cigar is even better than a cigarette. It would thus appear to be a peculiarity of the tobacco rather than of the paper of the cigarette. But we cannot say that it is a peculiarity of burning tobacco alone, as there may be other substances which will show the same effect if tested.

We come next to what may be regarded as physiological or psychological explanations, since we seem to have exhausted a purely physical type of explanation. The first guess that one is inclined to make is that we have to do here with a fatigue or adaptation effect. Fatiguing or adapting to red should make us more sensitive to green. If this is true, why do we not see our color discs as green under red light when we rotate red and green on the color

mixer? We should also expect, on this hypothesis, to see our blue-yellow disc as blue under yellow illumination, or our cigarette as yellow under blue illumination, both of which do not happen. We are certain, however, that we are not dealing with a fatigue or adaptation effect, because: (1) we see as good a green the moment the red light is turned on as after many minutes of exposure to red; (2) if the eye is screened from the red light so that only the glowing end of the cigarette is visible, the effect is even better.

A second psychological explanation is that we have to do with a contrast effect whereby the glow of the stimulus appears in the contrasting hue to that of the background of red. This hypothesis fails to satisfy for several reasons: (1) the green appears best not in the shaded portions of the ash but where light is actually coming from the cigarette; (2) when no background of red is allowed to reach the eye, the end of the cigarette alone being visible, we still get the green effect; (3) we should expect under blue illumination to see as striking a yellow as we do green under red illumination, but this does not occur; it is true, however, that under green illumination the cigarette appears much redder than under any other conditions. But the fact that we get a reversed effect, so to speak, does not mean that the contrast theory explains either.

A third explanation that occurs to the psychologist is that we may be dealing here with some sort of phenomenon having to do with dark adaptation. It is known that in dark adaptation there is not only a shift in brightness (Purkinje effect) but also a change in the relative values of the hues. We do not find that dark adaptation makes any difference: the green appears as well with no dark adaptation as after one-half hour of dark adaptation. The building up of the visual purple has nothing whatever to do with our phenomenon.

That this is not a purely subjective phenomenon can hardly be doubted by anyone who takes the trouble to observe the effect for himself. A striking exception to our statement that all observers see the glow as green furnishes a strong argument for the peripheral origin of our phenomenon: in a group of persons where we tried to demonstrate this illusion under very unfavorable conditions, the only one who failed to see any green at all in the glow was a deuteranope! This seems to point to the eye as the seat of the effect.

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